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ABSTRACT

A collection of papers on linguistics includes: "Italian Comparatives of Inequality with 'Che' and 'Di'" (Adrian C. Battye); "A Note on Passives in GPSG [Generalized Phrase Structure Grammar]" (Robert D. Borsley); "Reduplicated Constructions in Chinese and Questions of Generative Power" (Jonathan Calder); "A Note on Subcategorization, Constituent Order, and the Notion 'Head'" (Gerald Gazdar, Geoffrey K. Pullum); "Constituent Structure in Text-Copying" (Patrick D. Griffiths); "Gaps in Generalised Phrase Structure Grammar" (Steve J. Harlow); "Some Rhythm, Resonance and Quality Variations in Urban Tyneside Speech" (John K. Local); "A Methodology for Describing Socio-Linguistic Variability within Multi-lingual Settings in General and 'Interactive' and 'Reactive' Ethnic Processes in Language in Particular" (Ben Rampton); and "Ellipsis Conditions and the Status of the English Copula" (Anthony R. Warner). Three monographs are also reviewed. The monographs are on sentence particles in Cantonese, interracial friendship and communication among black and white adolescents, and Pacific linguistics. (MSE)

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ITALIAN COMPARATIVES OF INEQUALITY WITH CHE AND DI.*

Adrian C Battye

(Department of Language and Linguistic Science)

1.0 The Basic Data

The object of this paper is to examine a specific set of comparative structures in Italian, those which are termed comparatives of inequality. For ease of reference it will be considered that comparative structures of this kind are constructed along the lines shown here:

$[_{XP}$ or $[_{S}$ ... Quantifier X′$[_{\alpha}$ Introducer $[_{\alpha}$Target of]]
head comparison

The head of a comparative structure may be a phrasal constituent of any syntactic type (i.e. Adv., Adj., Noun, Verb) which is modifed by a quantifier of adverbial or adjectival status or an S which ultimately contains a quantified XP. The head of the comparison is followed by a complement whose exact syntactic status must be left vague at this stage, hence the label α ; it is this complement which will be referred to loosely as the target of comparison. The target of comparison in Italian can be introduced by the two lexical items che and di; at this stage these will be loosely referred to as introducers, meaning quite simply that their function in this syntactic structure is to introduce the target of comparison.

In Italian, comparatives of inequality are marked by the quantifier adjective/adverbs più (more) and meno (less). In one type of comparative structure in which these items are found the target of comparison is introduced by the lexical item che as in the example given under (1):

(1)(a) Maria è più intelligente che bella.
M is more intelligent than beautiful

(b) C'è meno gioia che tristezza.
there is less joy than sorrow

In the terms of the syntactic model in which this paper is cast, the data shown in (1) are taken as being instantiations permitted by one of the comparative rules. The data in (2) illustrate a second kind of comparative structure to be found in Italian:

(2)(a) Giovanni è meno intelligente di Giacomo.
G is less intelligent than G

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(b) La crema è più gustosa della torta[1]
the cream is more tasty than-the cake

In these two examples the same two quantifiers are to be found as in the structures in (1), but instead of the target of comparison being introduced by *che*, there is a constituent which in its surface forms appears very much like a PP introduced by the preposition *di* (of). Examples with the target of comparison introduced by *di* are to be instantiated by a comparative rule which is distinct from that which instantiates structures of the kind shown in (1).

1.1 A Semantic Difference?

Before giving a syntactic analysis of these two structures, it would be wise to dispense with one line of reasoning which might seek to distinguish these different structures on semantic grounds. It is not beyond the bounds of possibility that *che* and *di* might correspond to different scope possibilities for comparative structures (see Pinkham 1982 pp. 86-94) for a justification of the notion of scope with respect to comparative structures). However if comparative structures with *che* were semantically distinct from those with *di*, then it would be predicted that they would never be in free variation in contexts which were otherwise synonymous. This prediction is false because such contexts do indeed exist:

(3) (a) Apprezza più le stampe {che i quadri
{dei quadri
(Lepschy and Lepschy 1977: 107).
(he) appreciates more (the) prints than (the) paintings

(b) C'è più vento questo mese {del mese scorso.
{che il mese scorso.
there's more wind this month than (the) last month

(c) Mi piace più la carne {che il pesce.
{del pesce. (ibid.)
to-me pleases more the meat than the fish

In fact there does seem to be a degree of interchangeability which usually allows *di* to be replaced by *che* (but not vice versa). Some of the resulting configurations are considered however to be odd, if not out and out ungrammatical:

(4) (a) ? Mario è più bello che Maurizio.
Mario è più bello di Maurizio.
M is more handsome than M

(b) ? Claudio mangia più carne che il suo cane.
Claudio mangia più carne del suo cane.
eats more meat than the his dog

Also when an oblique pronoun follows *di* introducing a target of comparison, then *di* and *che* are no longer in free variation:

(5) (a) Giovanni mangia più pane di te / di me.
G eats more bread than you / than him

(b) *Giovanni mangia più pane che te/ che me.

In later discussion it will be shown that the problems encountered in (5) can be accounted for syntactically. The preference for *di* in (4) is not considered to be due to the syntactic structures involved but rather to *stylistic* factors. Indeed in certain registers of Italian, which Fornaciari 1972: 352 refers to as "nobile ed elegante" (noble and polished), it is claimed that the possibility of *che* introducing the target of comparison is preferred to that of *di*. Unfortunately the only examples to be found are those given by Fornaciari himself and these are taken from 15th century literary texts, so their value with respect to usage in the contemporary language is somewhat dubious. Here are the examples anyway:

(6) (a) Una donna più bella assai che'l sole
a woman more beautiful truly than the sun
(possible also: Una donna più bella assai del sole)
(Petrarch, cit. Fornaciari)

(b) Pareami ch'ella fosse più che la neve bianca
(it)seemed to-me that she was more than the snow white
(more natural today: Mi pareva che ella
fosse più bianca della neve)
(Boccaccio, cit. ibid.).

In as far as stylistic differences are not to be considered semantic differences and considering that the scope relations remain the same if *che* is substituted for *di* in all the examples in (3), (4) and (6), then it seems justifiable to say that these two introducers do not affect the semantics of strings in which they appear.

A final set of data (also mentioned in Fornaciari) which confirm this conclusion are contexts where *che* is preferred to *di* because the use of *di* would produce ambiguity, that is to say where the use of *di* could be interpreted either as a PP complement to a quantified head noun or adjective, or as the target complement of a comparative. Examples are shown in (7) where the comparative reading in (b), (d) and (f) is to be preferred:

(7) (a) Io sono meno soddisfatto di Giulia.
I am less satisfied than G
or I am less satisfied with G

(b) Io sono meno soddisfatto che Giulia.
I am less satisfied than G

(c) Venderanno più libri di Umberto Eco.
(they)'ll sell more books than U E
or (they)'ll sell more books of U E

(d) Venderanno più libri che Umberto Eco.
(they)'ll sell more books than U E

(è) Mi piace di più la sorella di Giovanni.
to-me pleases more the sister than G
or to-me pleases more the sister of G

(f) Mi piace di più la sorella che Giovanni
to-me pleases more the sister than G

These data can be taken as confirming the view set out above that where the comparative rule instantiating *di* as an introducer is possible there is also an alternative string instantiated by the comparative rule introducing *che*. The alternative using *di* may in certain styles and registers be the preferred one, but the use of *che* is acceptable when ambiguity may arise (see further discussion of this point in 3.1.1.1). Thus it is to be concluded that there is no semantic difference between comparative structures with introducers *di* or *che*, and this conclusion is taken as a justification of the approach of this paper which is to propose that Italian has two syntactically distinct ways of instantiating the comparative of inequality. The question of the semantics of this comparative structure in Italian will not be dealt with here, but it is considered that an approach similar to that put forward in Klein 1980 could be adapted to this language. Incidentally the idea that languages may have more than one syntactic process for comparative formation is not new in literature of modern Linguistics (see Hankamer 1973 and Pinkham 1982).

2.0 Comparative Rules for the Introducer CHE

The syntactic framework in which this study is cast is that of Generalised Phrase Structure Grammar as laid out in Gazdar and Pullum 1982 and in particular Gazdar 1980 where the comparative structures of English are specifically dealt with. An awareness of the basic concepts of this approach to grammar is assumed. The rules to be proposed in this section are those necessary for the instantiation of one type of comparative structure whose target of comparison is introduced by *che*. To deal with comparative structures using *che*, the following basic schema is proposed:

```
CAT ----) CAT    CAT[+/- SLASH]
                 [p]
```

In this rule schema CAT = any syntactic category, [*p*] represents a syntactic operator, in this case the *che* which introduces the target of comparison, and CAT[+/-SLASH] is a rather idiosyncratic way of showing in the most general way the possibility that in one class of *che* comparative structures in Italian a SLASH dependency could plausibly be argued for.

An advantage of this approach is that the analysis need not become overconcerned with the categorial status of the *che* found in comparatives: it is quite simply an operator which forms part of the compared phrase. This is the position adopted in Chomsky and Lasnik 1977 and Gazdar 1980, but for differing points of view on the status of the item introducing the target of comparison see Kayne (1976: 277-8, 297) on *que* in French which he considers to be a manifestation of the general complementizer of that language and Chomsky (1977: 88) where *than* in English comparatives is taken to be a preposition.

The first rule to be proposed is designed to deal with comparative structures of the kind shown in (8) below:

(8) (a) Maria è più bella che intelligente.
M is more beautiful than intelligent

(b) Più uomini che donne sono stati intervistati al
more men than women have been interviewed on the
telegiornale.
news bulletin

(c) Paolo scrive più rapidamente che correttamente.
P writes more rapidly than precisely

Examples of the type shown in (9) which, it is claimed, are different from those shown in (8), will be examined in 2.3:

(9)(a) Questo gravissimo incidente è stato più un errore
this very-grave accident has been more an error
delle autorità municipali che colpa degli inquilini
of-the authorities municipal than fault of the
tenants

(b) E' meno sulla tovaglia che sotto la tavola
(it)is less on the tablecloth than under the table
che si è sparso il vino.
that itself has spilled the wine

The following rule (based on the schema shown earlier) is proposed in order to instantiate structures of the kind shown in (8) (for proposals similar, though not identical to this one see Lozano and Pinkham 1984: 5):

CHE Comparative Rule 1
(first version)

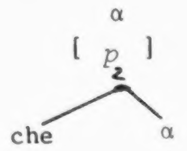
$$X \longrightarrow \underset{[p_1]}{X} \quad \underset{[p_2]}{\alpha}$$

where $p \in \{\langle \text{più, che}\rangle, \langle \text{meno, che}\rangle\}$

$X'' \in \{A'', N'', Adv''\}$,

$\alpha \in \{X''/\text{spec}X'\}$

By convention it is assumed here that double-bar categories are maximal projections (see Borsley 1983). The features $[p_1]$ $[p_2]$ are the first and second members of the ordered pairs più...che and meno...che. This Immediate Dominance (I.D.) rule can be interpreted as follows: a maximal lexical projection dominates two categories, one of which contains the feature $[p_2]$. The other category (α) will have to have the same kind of category features as the overall constituent with the exception that a SLASH feature is included among its specifications. The category marked 'α' will be instantiated with the feature $[p_2]$, giving trees like the following:



In order to understand more clearly the form of the phrase marked 'α' (i e the target of comparison), it is necessary to account for the fact that in comparative structures such as those in (8) the NP, AP or AdvP in the target of comparison cannot appear with any overt quantification, nor can the NP occur with determiners. The data in (10) illustrate these points:

(10) (a) *Più uomini che molte donne hanno fatto domanda.
more men than many women have made (an) application

(b) *Più uomini che le donne hanno fatto domanda.
more men than the women have made (an) application

(c) *Maria è più bella che tanto intelligente.
M is more lovely than so intelligent

(d) *Maria scrive meno rapidamente che molto correttamente
M writes less quickly than very correctly

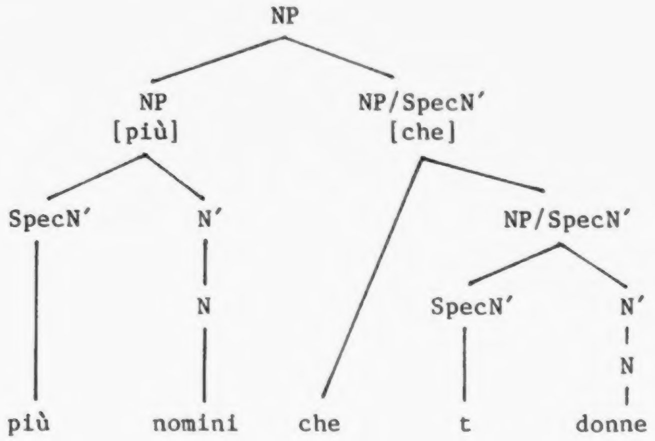
The impossibility of overt lexical material occurring in pre-head position in this type of structure's target of comparison suggests, as noted in Gazdar 1980, that the

SpecX′ node in the following rule should be interpreted as a SLASH dependency:

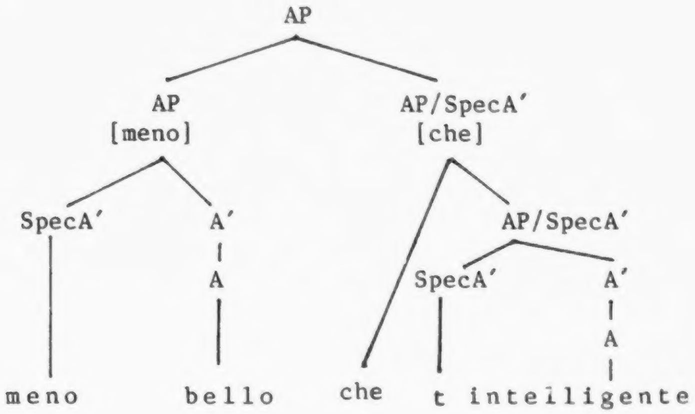
(11) X′′ ----) SpecX′ X′

Thus trees such as those shown in (12) would be instantiated with SLASH dependencies within the target of comparison:

(12) (a)



(b)



Because of the obligatory SLASH dependency shown here, it would be impossible to instantiate data such as those shown in (10).

The kinds of comparative being examined here are instantiated by rules of a similar kind (though not identical) to those which instantiate co-ordinate structures. Notice that the rule framework proposed here stipulates that the mother and the daughters must share a degree of categorial similarity. Indeed the Head Feature Convention(see Gazdar and Pullum 1982) should ensure that only categories sharing all feature markings in common with the exception of SLASH can co-occur in these configurations. It is assumed here, as has been proposed in Gazdar et al (1985: 165), that SLASH is a head feature. The comparative rule proposed above is not incompatible with the Head Feature Convention which only operates in the unmarked case and whose effects can be overridden by specific rule statements.

It is a consequence of this approach that strings such as the following are not to be found:

(13) (a) *Ci sono piú uomini che belli (NP + AP).
there are more men than handsome

(b) *Cantano più armoniosamente che stonati (AdvP + AP).
(they)sing more harmoniously than out of tune

(c) *Guida meno macchine che bene (NP + AdvP).
(he)drives less cars than well

Data such as these show that just as in coordinate structures, comparative structures of this kind require the mother and the daughter to share features in common, however the crucial use of a SLASH dependency in the target of comparison in this analysis means that a total conflation of co-ordination and comparative structures is not possible.[2]

One further prediction to be made concerning the comparative rule in its present form is that only three kinds of phrasal categories participate in its instantiations as it stands at present, namely AP, AdvP and NP. The need for this condition to be imposed on the type of comparative structure seen so far can be established empirically by data such as those in (14):

(14) (a) *[$_S$ [$_S$ Ho lavorato meno] [$_{S/Adv}$ che ho dormito]].
(I)'ve worked less than (I)'ve slept

(b) *Maria [$_{VP}$ [$_{VP}$ ha mangiato più pasta]
M has eaten more pasta
[$_{VP/NP}$ che ne ha digerito]].
than of-it has digested

(c) *Ho parlato [$_{PP}$ [$_{PP}$ con più professori]
(I)'ve spoken with more teachers
[$_{PP/Spec\ N'}$ che con studenti]].[3]
than with students

These three examples show respectively that ungrammaticality results in comparative structures of this type if Ss, VPs and PPs are the constituents conjoined by the comparative rule. Data such as (14) (illustrating comparatives of inequality) should not be taken as an indication that sentential constituents do not participate in other types of comparative structure which can be analysed along the same lines as co-ordinate structures. Consider for instance the following rule:

Sentential Comparative Rule: $X \longrightarrow \underset{[p_1]}{X} \quad \underset{[p_2]}{X}$

$$\text{where } p \in \{\langle \text{più, più} \rangle, \langle \text{più, meno} \rangle, \langle \text{tanto, quanto} \rangle\}$$
$$X \in \{S\}$$

A rule of this sort could be used to instantiate comparative structures of the type shown in (15):

(15) (a) Più Maria vede Mario, più ella lo apprezza.
more M sees M more she him likes

(b) Tanto era intelligente la madre, quanto era
so-much was intelligent the mother as-much was
scemo il figlio.
stupid the son

However, since the main concern of this paper is the structure of the comparative of inequality, a detailed analysis of sentential comparatives is not included.

2.1 "Comparative Ellipsis" or Rule 2 for CHE

The comparative structures examined in the previous section provide justification for the tie-up between comparative structures with *che* and co-ordinate structures in general. The data examined in the previous section, however, did not include examples of the type shown in (16):

(16) (a) Maria ha più libri che io dischi.
M has more books than I records

(b) Luigi ha più libri all'università che dischi
L has more books at university than records
a casa.
at home

(c) Gianni è meno attaccato a sua moglie che a
G is less attached to his wife than to
sua madre.
his mother

(d) Ho studiato dettagliatamente più commedie di
(I)'ve studied in detail more plays of
Goldoni che di Gozzi.
Goldoni than of Gozzi

Comparative structures of the kind shown in (16) are generally referred to as examples of 'comparative ellipsis' in the generative literature. Lozano and Pinkham (1984) argue convincingly against this traditional approach for equivalent Spanish structures; to deal with these kinds of comparative, they formulate instead a rule schema within the framework of GPSG. Their analysis postulates a rule which is very similar to the *Comparative Rule 1* formulated earlier, except in respect of the conditions to be attached to the constituents which can participate in comparative structures instantiated by this rule. In what follows, the Lozano and Pinkham analysis of Spanish data is adapted to the Italian data in (16). Again the basic rule schema is that which is associated with co-ordinate structures:

CHE Comparative Rule 2

$$\mathrm{X} \;\text{---)}\; \begin{array}{c}\mathrm{X}\\ {[p_1]}\end{array} \quad \begin{array}{c}\mathrm{X}\\ {[p_2]}\\ {[\mathrm{ELL}]}\end{array}$$

$$p \in \{\langle \text{più, che}\rangle, \langle \text{meno, che}\rangle\}$$

$$\mathrm{X} \in \{\mathrm{S, VP, PP, NP, AP, AdvP}\}$$

(based on Lozano and Pinkham 1984: 5)

This rule states that as with *CHE Comparative Rule 1*, a category X will dominate two categories of the same syntactic kind. The first of these two constituents will contain the comparative operator *più* or *meno* and the other will be introduced by the operator *che*. The feature which requires justification in the above schema is the [ELL]; this means that the target of comparison introduced by *che* in these kinds of comparative cannot be a full realisation of X, but must instead be an elliptical or partial realisation of this constituent. As Lozano and Pinkham state: "The elliptical X ... of this rule... will dominate a series of major focused categories, which have as their only property that they cannot be identical in category to X ... [ELL]"(ibid 6). A rule like the following is proposed:

$$\begin{array}{c}\mathrm{X}\\ {[\mathrm{ELL}]}\end{array} \;\text{-----)}\; \begin{array}{c}\mathrm{Y}\\ {[+\mathrm{FOC}]}\end{array} \qquad \text{where } \mathrm{X} \neq \mathrm{Y}$$

Essentially *CHE Comparative Rule 2* accounts for data like (17) which show that in constructions of this kind conjunctions of exactly the same constituents are not to be found:

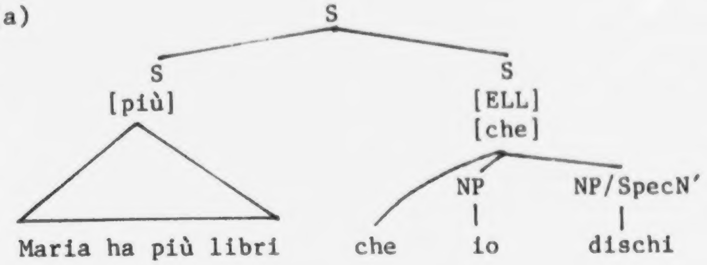
(17) (a) *$[_S$ $[_S$ Mario lavora più] $[_S$ che
M works more than

$[_S$ Paolo canta/lavora]]].
P sings/works

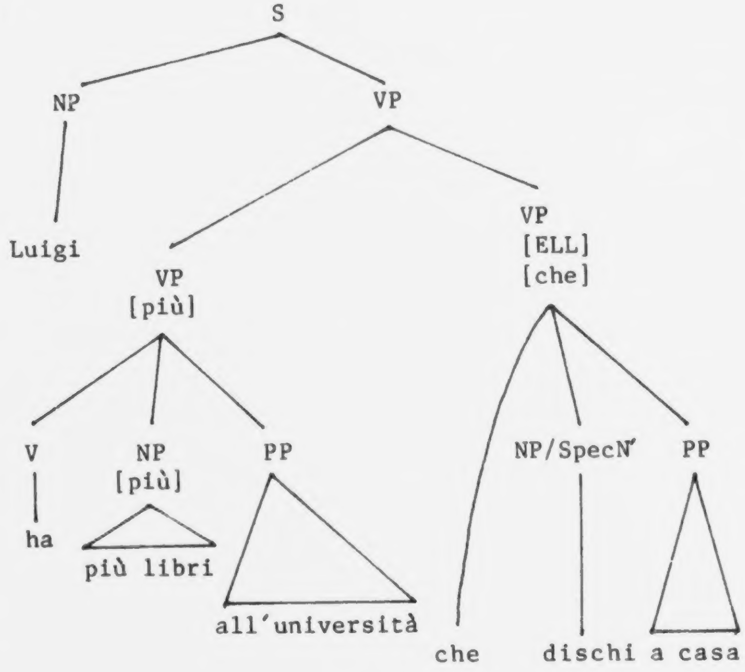
(b) *Maria [$_{VP}$ ha letto più libri] [$_{VP}$ che
M has read more books than
[$_{VP}$ ha scritto articoli]].
has written articles

If the target of comparison is elliptical in the way specified by the rules above, then grammaticality results and sentences such as those shown in (16) result. Under (18), some of the tree structures instantiated by this rule are given:

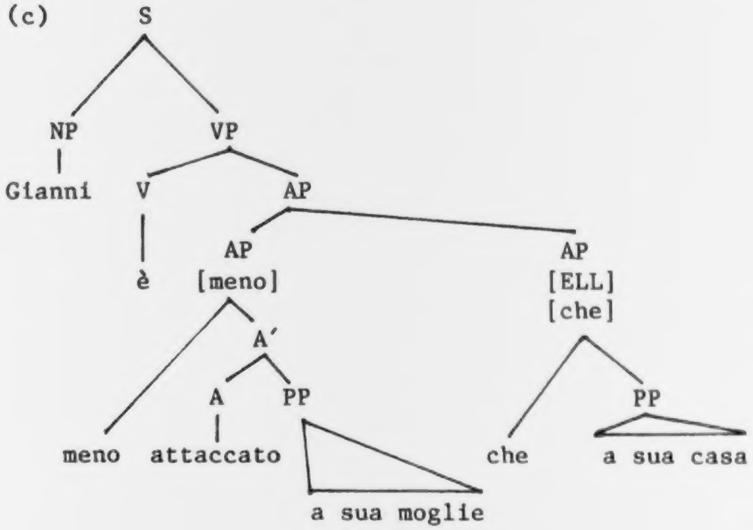
(18) (a)



(b)



(c)



2.2 Problematic PP and NP Comparative Structures

In the previous section it was pointed out that the data in (9) (repeated here for convenience) were not to be accounted for by the *CHE Comparative Rule 1* in its present form.

(9) (a) Questo gravissimo incidente è stato più un errore
this very-grave accident has been more an error
delle autorità municipali che colpa degli
of-the authorities municipal than fault of the
inquilini.
tenants

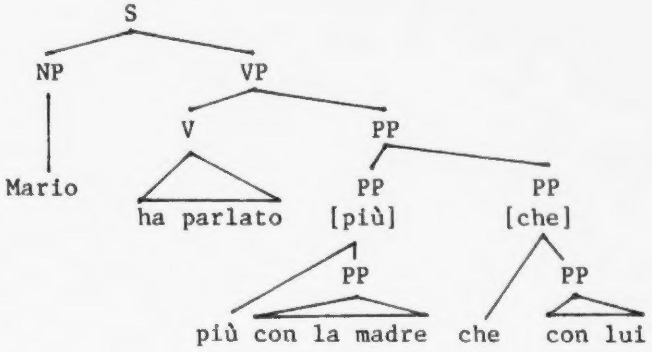
(b) E′ meno sulla tovaglia che sotto
(it)is less on-the tablecloth than under
la tavola che si è sparso il vino.
the table that itself has spilled the wine

A close examination of these two examples illustrates why structures of this kind are problematic for the rule in question (at least in its first version). In (9)(a) it is clear that the comparative quantifier *più* is not generated under the SpecN′ node as was the case in the tree (12)(a). Similarly (9)(b) is not instantiated by the rule as stated at present, because that rule makes no mention of PPs as being categories which participate in comparative structures of this kind. If a syntactically similar structure such as that in (19) is examined, it could be argued that the string is syntactically ambiguous:

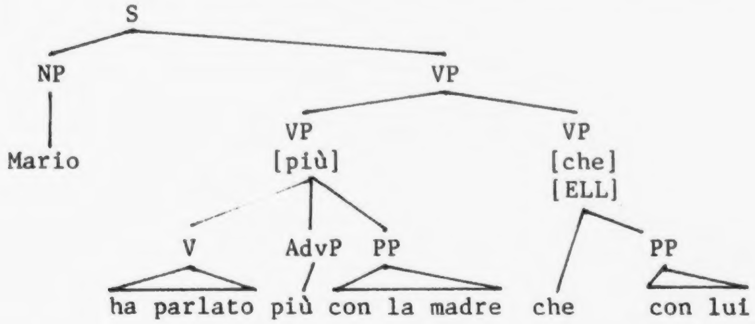
(19) Mario ha parlato più con la madre che con lui.
M has spoken more with the mother than with him

The surface structure shown here could be instantiated both by a modified version of Comparative Rule 1, giving the structure in (20), or by Comparative Rule 2, giving the tree structure in (21):

(20)



(21)



The distinct syntactic structures shown here can be intuitively felt to have different semantic interpretations. In (21) the scope of the comparative might loosely be said to include the verb (ha parlato: has spoken), while in the structural analysis given in (20) the comparative scope is restricted to the compared PPs. The structure in (20) also implies that the compared PPs form a constituent in their own right and therefore comparative PP structures of this type should be free to appear in the focus position of clefts, in dislocated structures, and in sentence fragments as do normal PPs. All these possibilities are illustrated in (22) below and it is worth noting that in these data only the reading in which the scope of the comparison is restricted to the PP is possible:

(22) Clefts

(a) E' più per la forma che per il fastidio che
(it)is more for the form than for the disgust that
mi dà che sto contestando la decisione.
(it)to-me gives that (I)am disputing the decision

(b) E' più nel suo giardino che all'università
(it)is more in his garden than at university
che vedo il mio capo.
that (I)see my boss

Dislocated Structures

(c) Più verso la piazza centrale che nella
more towards the square central than in+the
direzione della posta è dove
direction of+the post office is where
troverai i negozi.
(you)'ll find the shops

(d) Meno di sinistra che di destra mi sembrano
less of leftwing than of rightwing to-me seem
le idee politiche del nostro nuovo collega.
the ideas political of our new colleague

Sentence Fragments

(e) A. Ha sempre combattuto così contro il sistema?
(he)'s always fought so against the system

B.Non più contro di me personalmente che
no more against me personally than
contro il sistema in sè.
against the system in itself

(f) Meno con me che con sua madre, d'accordo, però
less with me then with his mother o.k. but
devo dire che si è comportato male anche
(I)must say that (himself) has behaved badly even
con me.
with me

The data in (22) are taken as indicative of the need to include PPs among the class of constituents which can participate in the CHE Comparative Rule 1. However, if PP was simply added to the class of structures which can be instantiated as heads by this rule, then there is apparently nothing in the formalism of the rule as it stands which would prevent the instantiation of ungrammatical strings such as (23):

(23) (a) *Ho parlato [$_{PP}$ [$_{PP}$ con più professori]
(I)'ve spoken with more teachers
[$_{PP/SpecN'}$ che [$_{PP/SpecN'}$ con studenti]]].
than with students

(b) *Ho fatto domanda a meno licei che a
(I)'ve made applications to fewer schools than to
scuole magistrali.
training schools

Suppose that the syntactic rules did instantiate comparative structures of the type illustrated in (23); such a position does not necessarily entail that the surface structure would be grammatical. Indeed the ungrammaticality of the strings in (23) may plausibly be explained by an independent constraint preventing the interpretation of empty categories contained within PPs. That PPs in Italian do not allow unbounded dependencies to pass through them is shown in the contrasting data here:

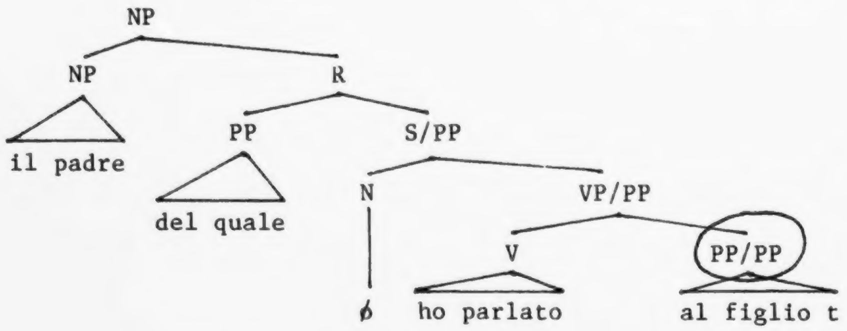
(24) (a) Il padre [$_{PP}$ del quale] [$_{S/PP}$ conosco
the father of whom (I) know
[$_{NP/PP}$ il figlio ---]]
the son

(b) Il padre [$_{NP}$ il figlio del quale]
the father the son of whom
[$_{S/NP}$ conosco ---]
(I) know

(c) Il padre [$_{PP}$ al figlio del quale]
the father to-the son of whom
[$_{S/PP}$ ho parlato ---]
(I)'ve spoken

(d) *Il padre [$_{PP}$ del quale] [$_{S/PP}$ ho parlato
the father of whom (I)'ve spoken
[$_{PP/PP}$ al figlio ----]]
to-the son

In order to account for the ungrammaticality in (24)(d), it might be proposed following Maling and Zaenen (1982) that a tree well-formedness condition of the from *PP/ α exists in Italian. This condition stops any dependency from passing through a PP node (a similar condition for French might be proposed based on the data discussed in Kayne 1975: 114). The same tree well-formedness condition, it might be maintained, could plausibly account for the ungrammatical data in (24)(d). As was said in the account given of the form of the target of comparison in comparatives instantiated by the present version of Rule 1, it is necessary to hypothesize the existence of an empty quantifier which modifies the head of the phrase. This empty position has been treated as a SLASH dependency instantiated between the node introducing the target of comparison and the head of the phrase (see Gazdar 1980: 172-77). Thus if we wrote out fully a tree structure for (24)(d) the following would result:



The circled node here is to be interpreted as a violation of the tree well-formedness condition which prevents SLASH being attached to PP. Interesting corroboration of this view (that it is the general islandhood of PPs in the Romance languages which prevents these kinds of comparative structures being instantiated) comes from French which, like Italian, does not allow structures equivalent to (24)(d):

(25) *Jean a parlé avec plus d'étudiants qu'avec
J has talked with more students than with
de professeurs
lecturers

On the other hand the syntax of English does not contain a similar well-formedness condition for PPs, hence the well known phenomenon of preposition stranding in that language. As a consequence of the possibility of allowing a SLASH dependency into PP, English also allows comparative structures whose syntax corresponds to that shown in (24)(d) and (25):

(26) (a) I spoke to more lecturers than to professors.
(b) He copied from less books than from newspapers.

Before considering the modifications which have to be made to the *CHE Comparative Rule 1*, it is necessary to show that structures such as those shown in (9) can also be accounted for within a single rule statement. NPs introduced by *più/meno* with overt lexical material other than these comparative quantifiers in SpecN' are grammatical in Italian and in (27) below there are a variety of such structures, all of which figure in contexts where only an overall NP analysis would be possible, but where SpecN' does not contain *più/meno*:

(27) (a) Sono più i tuoi libri che i suoi che mi piacciono.
(they)are more your books than his that to-me please

(b) E' più il tuo che tutti gli altri compiti
(it)'s more yours than all the other homeworks
che mi è rimasto impresso.
that to-me has remained impressed
(NP focus of Cleft Structure)

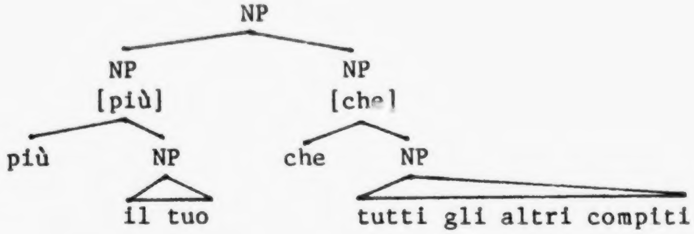
(c) A. Hai apprezzato tutti i quadri
(you)'veappreciated all the paintings
della mostra d'arte moderna?
of the exhibition of modern art

B.Si, però meno i quadri di Picasso
Yes but less the paintings of P
che quelli di Braque.
than those of Braque.
(Sentence Fragment)

(d) Più i cavoli che le coste sono stati
more the cabbage than the beets have been
toccati dal gelo.
touched by-the frost (Subject NP)

It would seem that in (27) are found cases of comparative structures in which full NPs are conjoined. Notice in particular the NP of (27)(b) with the target of comparison tutti gli altri compiti; such a structure shows all the possible preN positions filled by lexical material. Therefore in an example such as this there can be no SLASH dependency into SpecN', and a structure such as (28) is proposed which resembles the structure (20) shown above for similar PP headed structures:

(28)



How then might structures such as (20) and (28) be incorporated into the CHE Comparative Rule 1? It is proposed that the form of the target of comparison be modified to allow not only X''/SpecX' categories to occur but also X'' constituents without SLASH. Therefore the final version of the rule will be:

CHE Comparative Rule 1
(final version)

$$X \dashrightarrow \underset{[p_2]}{X} \quad \underset{[p_2]}{\alpha}$$

where $p \in \{\langle \text{più, che} \rangle, \langle \text{meno, che} \rangle\}$

$X \in \{A'', N'', Adv'', P''\}$

$\alpha \in \{X''/\text{SpecX}', X''\}$

As formulated, this rule would instantiate a string like:

(29) *Ho parlato con più professori che con studenti.
(I)'ve spoken with more teachers than with students

But, of course, this string would be ruled out independently by the *PP/α tree well-formedness condition discussed above.

The remaining problem with the formulation as it stands is that of 'mismatch' like in (30) where the head of the comparative has the quantifier *più* in the SpecN' position in the head and a full NP in the target of comparision:

(30) *Più uomini che tante donne hanno fatto domanda.
more men than so-many women have made applications

Structures such as these might plausibly be argued to be semantically ill-formed because of the absence of a SLASH dependency in the target of comparison. It is difficult to create convincing examples of the other kind of 'mismatch', namely that of *più* modifying a full N'' followed by a target of comparison of the form N''/SpecN'. Consider the examples in (31):

(31) (a) Ho mangiato più caramelle che dolci.
(I)'ve eaten more sweets than cakes

(b) Ho mangiato più le tue caramelle che i tuoi dolci.
(I)'ve eaten more your sweets than your cakes

(c) *?Ho mangiato più le tue caramelle che dolci[4]
(I)'ve eaten more your sweets than cakes

In Italian noun phrases consisting of a noun without any determiner are possible and these configurations generally have an indefinite interpretation. In (31)(c) it is possible to interpret *dolci* as just such an indefinite noun phrase, although this interpretation is somewhat unnatural (hence the grammaticality judgement). In accordance with the predictions made by this analysis that no SLASH dependency would be possible into the target of comparision in (31) (c), it is impossible to interpret *dolci* here in the same way as in (31) (a).

A further prediction made by this rule is that APs and AdvPs which are already modified by adverbials (the only constituents which can occur in specifier position in APs and AdvPs) might also be found in comparative constructions. Unfortunately it is very difficult to test for structures of this type because in general the adverbials found in specifier position in AP and AdvP are quantifiers and there are very strict restrictions on the co-occurrence of quantifiers within one single constituent. However it is plausible that structures like (32) are illustrative of targets of comparison which do not contain a SLASHED AP:

(32) (a) Sono più stanco che proprio distrutto.
(I)am more tired than really done-in

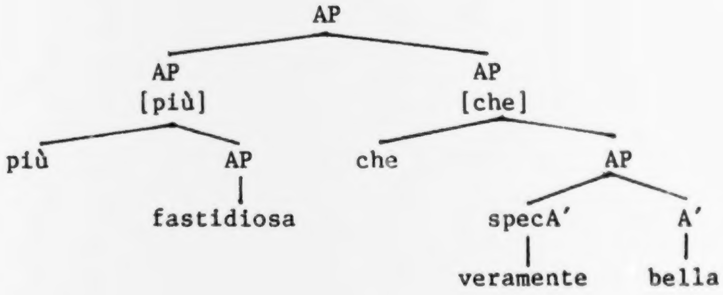
(b) E' stata una serata più faticosa che
(it)'s has been an evening more tiring than
veramente bella.
really pleasant

To illustrate that there is probably no SLASH in the target of comparison here, data such as (33) might be cited which show that an adjective modified by an adverbial such as proprio or veramente cannot be modified in turn by a quantifier:

(33) (a) Proprio distrutto (b) *Più/meno proprio distrutto
really done-in more/less really done-in
(c) Veramente bella (d) *Piu/meno veramente bella.
truly beautiful more/less truly beautiful

Therefore a tree structure like the one shown in (34) seems the most plausible for these examples:

(34)



A further set of data which potentially illustrate this type of comparative (where there is no SLASH in the target and where the head is a maximal lexical projection modified by the quantifier più) are these conjunctions of superlatives:

(35) (a) ?Mario è piùfurbissimo che proprio
M is more very-crafty than really
intelligentissimo.
very-intelligent

(b) ?E' una ragazza che mi pare più
(she)'s a girl who to-me seems more
truccattissima che veramente bellissima.
very-made up than truly very-beautiful

(Strict purists may frown upon the apparent illogicality of modifying a superlative in any way; in order to indicate this the examples are marked ?', although informants generally do accept these strings.) Used independently, superlatives cannot be modified by adverbial quantifiers:

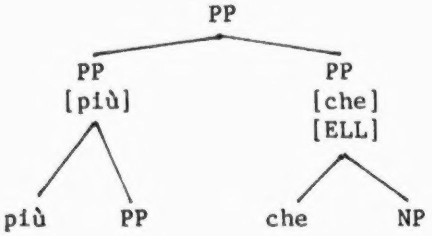
(36) (a) *Più furbissima
more very-crafty
(b) *Molto furbissima
very very-crafty

(c) *Tanto bellissima
much very-beautiful
(d) *Meno bellissima
less very-beautiful

Therefore it might be concluded that a comparative containing a superlative in one of its conjuncts is made up of two maximal lexical projections of AP, a possibility which is predicted in the final version of the CHE Comparative Rule 1.

Finally, in this section a problem with respect to PPs which as yet cannot be resolved might be signalled. CHE Comparative Rule 2, it might be predicted, should produce structures which have schematically the following form:

(37)



However structures of this type are less than perfect, and there is a definite preference for structures with no ellipsis of the preposition, although informant judgements tend to vary somewhat depending on the preposition used. Consider the data in (38):

(38) (a) ??E' più da sua madre che suo padre
(it)'s more by his mother than his father
che Giorgio è stato influenzato.
that G has been influenced

(b) (preferred reading of (a)) E' più da sua madre
che da suo padre che Giorgio è stato influenzato

(c) ?E' più come attore che autore   che quel
(it)'s more as actor than author that that
personaggio è celebre.
person   is famous

(d) (preferred reading of (c)) E' più come attore
che come autore che quel personaggio è celebre.

3.0 Comparative Structures with DI

In the introduction, attention was drawn to the existence of a type of comparative structure in which the target of comparison is introduced by *di* and not by *che*. In this section it is to be demonstrated that these types of comparative structure are instantiated by a rule schema which is distinct from that used for the *Comparative Rules 1* and *2* seen in Section 2. Therefore the crucial proposal in this part of the survey is that a third rule for the syntax of comparatives of inequality must be postulated in order to account for data of the form shown in (39):

(39) (a) Maria è più alta di Paolo.
M is more tall than P

(b) Maria studia più seriamente il francese
M   studies more seriously (the) French
di   Giovanni.
than G

(c) C'è   più vento oggi di   ieri.
(there)'s more wind today than yesterday

What is the category status of the targets of comparison shown here? *Di* is generally analysed as a preposition and a number of factors in the behaviour of targets of comparison introduced by *di* point in the direction of these constituents being PPs in status. Firstly prepositions in Italian do not assign nominative but oblique case; therefore the overt nominative pronouns of Italian (*io*, *tu*, etc...) cannot be governed by a preposition, but the corresponding oblique forms (*me*, *te* etc...) are found in such configurations. This general characteristic of PPs is also to be found in comparative targets of the type which interest us here:

(40) (a) Maria è più intelligente di *io/me.
M   is more intelligent than I/me

(b) Gli altri hanno lavorato più seriamente di *tu/te.
the others have worked more seriously than you/you

Data such as (40), besides giving justification to the view that the targets of comparison here are PPs, could also be used to argue against the proposal (potentially possible) that di targets of comparison are reduced clauses. If were reduced clauses then the pronouns in (40) (which are interpreted as subject arguments) would be expected to appear with nominative case marking.

Although the rules proposed in Section 2 do predict the possibility of structures like (41), informants do not like them, preferring the strategy which instantiates a target of comparison with di:

(41) (a) *Maria è più intelligente che io.
M is more intelligent than I

(b) *Paolo è meno intelligente che tu.
P is less intelligent than you

However, targets of comparison introduced by the item che can contain non in stylistically marked registers. The presence of this negative operator seems to improve data like those in (41):

(42) (a) Maria è più intelligente che non io.
M is more intelligent than (not) I

(b) Paolo è meno intelligente che non tu.
P is less intelligent than (not) you

That the pronominal forms here are nominative and not oblique is a strong argument against giving che the same prepositional status in comparatives as di. Here indeed a reduced clause analysis (like that given by CHE Comparative Rule 2 would seem appropriate

A further characteristic of PPs which might be invoked at this point is the simple distributional fact that prepositions do not govern APs or AdvPs but only NPs (laying aside structures such as contro di me (against me) for ease of exposition). This generalisation is found to hold with respect to targets of comparison which are introduced by di:

(43) (a) Mi piace più la pasta de[$_{NP}$ l dolce].
to-me pleases more the pasta than the cake

(b) C'era più vento il mese scorso di [$_{NP}$ questo mese].
(there)was more wind the month last than this month

(c) *E' più intelligente di [$_{AP}$ studioso].
(he)'s more intelligent than studious

(d) *Lavorava più rapidamente di [$_{AdvP}$ seriamente].
(he)worked more rapidly than seriously

(e) *Parla più spesso con suo padre di [$_{PP}$ con lui].
(he)speaks more often with his father than with him

That it is the conditions on the type of constituent which di can govern which causes the structures in (43) to be ungrammatical and not any special conditions to be attached to the form of the comparative clauses is shown by the data in (44), where similar comparative structures with targets introduced by che are possible (no English gloss is included: see glosses in (43)):

(44) (a) Mi piace più la pasta che il dolce.

(b) C'era più vento il mese scorso che questo mese.

(c) E' più intelligente che studioso.

(d) Lavorava più rapidamente che seriamente.

(e) Parla più spesso con suo padre che con lui.

Finally the behaviour of comparative targets introduced by di with respect to WH constuctions is again to some extent indicative of their PP status. This is the conclusion to be drawn from data like (45) which show that targets of comparison introduced by di can be fronted to form WH questions; this is a general characteristic of all phrasal categories in Italian:

(45) (a) Di chi è più alto Mario ____ ?
than whom is more tall M

(b) Di quale film sembra più interessante
than which film seems more interesting
quel libro ---?
that book

There are however no equivalent relative data for these shown in (45):

(46) (a) * Mario, di cui Paolo lavora più
M than whom P works more
seriamente ---, è bravo
seriously is clever

(b) *Quella poesia della quale non c'è niente
that poem than which (not) there is nothing
di più bello.
more lovely

Why there is this gap in the WH-paradigm cannot as yet be explained.

In contrast to the data shown in (45), targets of comparison introduced by che are never potential targets in

WH constuctions, a feature which can be construed as meaning that unlike di introduced targets of comparison, these ones are not phrasal in category status:

(47) (a) *Che con chi esce più spesso
than with whom (she)goes out more often
con Rodolfo ---?
with R
(O.K. Esce più spesso con Rodolfo che con me.
(she)goes out more often with R than with me)

(b) *Che quanto pratica era più geniale
than how practical was more genial
l'idea ---?
the idea
(O.K. L'idea era più geniale che pratica.
the idea was more genial than practical)

3.1 A Comparative Rule Introducing DI

It is proposed that comparative stuctures with di of the type examined in 3.0 will be instantiated by a rule of the following form:

$$X'' \text{ ----) } \underset{[p]}{X''} \ \underset{[\text{di}]}{P''}$$

X = any phrasal category
$p \in \{\langle\text{più}\rangle, \langle\text{meno}\rangle\}$

This rule is designed to capture the generalisation that comparatives of inequality can realize their target of comparison with a P'' introduced by the preposition di. The reason for writing the rule as being instantiated at the level of X'' is that the whole comparative phrase (head plus target) can be found to behave as a single constituent with respect to focussing (48) and clefting (49):

(48) (a) Più spesso di Giovanni ci sono andato io.
more often than G there have gone I

(b) Meno intelligente di Paolo sembra la nostra
less intelligent than P seems (the) our
piccola Maria
little M

(49) (a) E' più spesso di Giovanni che ci
(it)'s more often than G that there
sono andato io.
have gone I

(b) E' meno intelligente di Paolo che sembra
(it)'s less intelligent than P that seems
la nostra piccola Maria.
(the) our little M

Data such as these show that the *di* target of comparison is syntactically part of the comparative phrase. The semantics of the rule will however require that the target of comparison be interpreted as having a noun phrase role in the sentence which immediately contains the X″ instantiated in the above rule. Consider the following interpretations:

(50) Comparative structure

(a) Maria è più intelligente di Elena.
M is more intelligent than E

(b) Mangio la pasta piu volentieri del dolce.
(I) eat pasta more willingly than cake

(c) Hanno più scelta questo mese del mese scorso .
(they)'ve more choice this month than month last

Interpretation of the *di* Target

(a)′ Elena è (quantifier) intelligente.
E is intelligent

(b)′ Mangio (quantifier) volentieri il dolce.
(I) eat willingly the cake

(c)′ Avevano (quantifier) scelta il mese scorso.
(they)had choice the month last

Therefore the semantics of this rule (a question not being dealt with explicitly here) will require information about the sentence containing the X″ comparative phrase in order to permit a full interpretation to the PP[di] target of comparison. It is worth underlining at this point that *di* can be used to introduce any target of comparison which could be interpreted as meaningful in the sentence containing the comparative; that is to say such an NP can have subject, object or adverbial function (providing the adverbial is of NP form). It might be added at this point that the *che* of relative structures shares an identical distribution (see Cinque 1981).

3.1.1 Restrictions on the distribution of comparative targets introduced by DI

3.1.1.1 Collocation Restrictions

It seems that the target of comparison introduced by *di* can be interpreted incorrectly if it follows certain NPs or APs. The tendency in such cases is to interpret the *di* phrase not as a target of comparison but as a complement to the preceding head. Consider the pair of sentences in (51):

(51) (a) Maria studia più seriamente di Claudio.
M studies more seriously than C

(b) ??Maria studia più seriamente il francese
(i)M studies more seriously the French
(ii)M studies more seriously the French
di Claudio
of C
than C

The example (51) (a) is perfectly acceptable but (51) (b), which essentially has the same syntactic structure with respect the the comparative rule, is considered to be somewhat ambiguous and not perfectly acceptable as a comparative. This ambiguity is due to the fact that the PP *di Claudio* is more naturally interpreted as a complement of the NP *il francese*. A simple change of word order can resolve this problem of interpretation:

(52) Maria studia il francese più seriamente di Claudio.
M studies the French more seriously than C

The same problems can be identified and resolved by word order changes in the following example:

(53) (a) ??Giovanni teme più l'amore dell' odio.
G fears more love than/of hate

(b) Giovanni teme l'amore più dell'odio.
G fears love more than hate

That what makes the first of the sentences less than perfect is simply a question of surface ambiguity is illustrated by data like these in (54) in which the collocation of NP and *di NP* could not be ambiguous and in which both word orders are considered equally acceptable:

(54) (a) Ho visto più spesso Mario di sua madre.
(I)'ve seen more often M than his mother

(b) Ho visto Mario più spesso di sua madre.
(I)'ve seen M more often than his mother

(c) Ho visitato più volentieri gli Uffizi di
(I)'ve visited more willingly the Uffizi than
palazzo Pitti.
Pitti Palace

(d) Ho visitato gli Uffizi più volentieri di
(I)'ve visited the Uffizi more willingly than
palazzo Pitti
Pitti Palace

It is interesting to note that when ambiguity of the *di* comparative arises because of word order problems, there is a definite preference among informants to formulate the sentence with *che* introducing the target (see also (7)):

(55) Giovanni teme più l'amore che l'odio.
G fears more love than hate

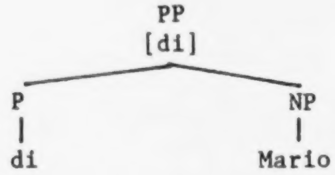
An interesting hypothesis to draw from these data is that speakers have the following strategy with respect to comparatives: the rule introducing *di* targets is to be preferred, but when it cannot apply, for instance because of the non-NP status of the target or when its application would produce surface ambiguity, then the rules introducing *che* in comparative targets are used.

3.1.1.2 An Apparent Gap in the Paradigm of *DI* Comparatives?

So far only *di* comparative targets of the type shown in (56) have been examined:

(56) (a) Maria è più grossa di Mario.
M is more big than M

(b)



The data in (57) illustrate examples of comparative structure whose targets of comparison contain SLASHED noun phrases. In these cases it is to be remarked that there are no equivalent structures with *di*:

(57) (a) Sono stati letti più libri che articoli.
have been read more books than articles

(b) *Sono stati letti più libri di articoli.
have been read more books than articles

(c) Marco ha mangiato più pasta che formaggio.
M has eaten more pasta than cheese

(d) *Marco ha mangiato più pasta di formaggio.
M has eaten more pasta than cheese

This difference in behaviour can be dealt with by reference to the well-formedness condition imposed on PPs which forbids them to be part of a SLASH dependency (see discussion in 2.1). As (56) shows, comparative targets with *di* are syntactically PPs, but the examples in (57) show comparative targets which require a SLASH dependency. This can be instantiated with no problem by using the *CHE*

Comparative Rule 1, giving a structure of the following form:

$[_{NP}\ [_{NP}$ più libri] $[_{NP/SpecN'}$ che $[_{NP/SpecN'}$ t articoli]]]

On the other hand a structure such as (56)(b) cannot be instantiated firstly because the rule instantiating *di* comparatives states that the target of comparison in these cases is a PP and nothing else, and secondly because to allow a SLASHED PP would constitute a violation of the well-formedness condition *PP/ α shown by the circled node in the following :

$[_{NP}\ [_{NP}$ più libri] $[_{\boxed{PP/SpecN'}}$ di $[_{NP/SpecN'}$ t articoli]]

Therefore the data in (56) are not a problem for the analysis proposed here and indeed the array of grammatical and ungrammatical structures follows as a natural consequence of the rule systems already proposed.

FOOTNOTES

* I recognise my debt to J. Pinkham whose work on comparative structures in French, Spanish and English (see Pinkham 1982 and Lozano and Pinkham 1984) has been of crucial importance in helping me organise the Italian comparative data. I would like to thank Steve Harlow and Anthony Warner for their help in preparing this article. I am particularly grateful to Simona Rizzardi and Giulio Lepschy for discussion of the data presented. All errors and inaccuracies are my own.

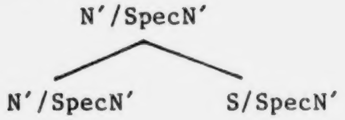
1 The preposition *di* (of) in Italian combines with the forms of a following definite article (*il*, *la*, *i*, *le*) to produce the following forms: di + il(masc. sing.) = del, di + la(fem. sing.) = della, di + i(masc. pl.) = dei, di + le(fem. pl.) = delle.

2 This analysis leaves open the possibility that, at a very abstract level, comparative and coordination structures may share the same basic rule schema. This possibility was hinted at by G. Gazdar in a talk given at the LAGB 1984 Spring meeting at Hull.

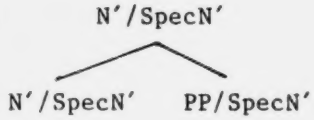
3 This ungrammatical string should be distinguished from the grammatical *Ho parlato più con professori che con studenti* (see section 2.3). The close similarity of (14)(c) and this example may explain why (14)(c), although considered ungrammatical, is not judged by informants to be as "bad" as (14) (a) and (b).

4 Intuitively it is being claimed here that the two parts of the comparative must be symmetrical; that is to say a maximal lexical projection quantified by a comparative operator must have a target of comparison which is a maximal lexical projection and in a similar fashion an N'', A'' and Adv'' containing a comparative operator in its SpecX' must be paired with a SLASHED target of comparison for it to be interpretable.
The idea that symmetry is required between the two parts of a comparative would deal with a difficulty brought to my attention by Anthony Warner. By analysing SLASH as a head feature, my analysis not only allows SLASH to be instantiated on the Head of the target of comparison, but also, theoretically at least, on any complement to the head. Therefore targets of comparison of the following kind might be instantiated:

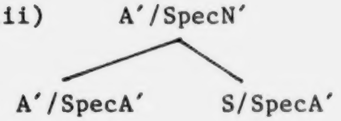
(i)



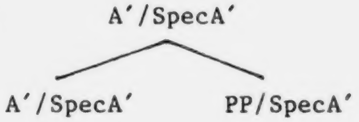
(ii)



(iii)



(iv)



Some of these structures could, however, be ruled out by the grammar for independent reasons. (i) and (ii) could be dealt with by whatever mechanism is proposed for the grammar to deal with data traditionally covered by the 'Complex Noun Phrase' constraint. (ii) and (iv) would also be ruled out by the well formedness condition preventing dependencies into PP, (iii) remains a problem. If instead of treating these examples separately, we invoked the claim that the two parts of the comparative structure must be symmetrical then we have an independent reason for not allowing a second SLASH dependency to be introduced into the target of comparison.

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A NOTE ON PASSIVES IN GPSG*

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Passives have been an important concern for generalized phrase structure grammar (GPSG) since the earliest work in the framework. In a number of publications, it has been proposed that their properties are largely a consequence of a metarule deriving rules for passive VP's from rules for active VP's. In the earliest work, the crucial rules are phrase structure (PS) rules. More recently, they are immediate dominance (ID) rules. In Gazdar et al (1985), the following metarule is suggested:

(1) VP ---> W, NP

$$\Downarrow$$

VP[PAS] ---> W, (PP[by])

In some of the earlier work, eg Gazdar (1982), passive participles have the same semantics as related verbs, but related active and passive VP's are associated with different semantic rules, the rules for passive VP's being derived from the rules for active VP's by an extended metarule. In Gazdar et al (1985), passive participles have different semantics from related verbs. This is handled by a lexical rule which derives passive participles from active verbs. In this note, I will outline two problems for this analysis of passives. I will propose a solution (of sorts) for the first, but I will have no solution to offer for the second. I will suggest in fact that the government binding framework (GB) is more satisfactory in this area.

An important fact about passives is that not all sentences where the verb takes a following NP have a passive counterpart. The following illustrate:

(2)a. John promised Mary to be on time.

b. *Mary was promised by John to be on time.

(3)a. The baby weighed eight pounds.

b. *Eight pounds was weighed by the baby.

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(4)a. The film lasted three hours.

b. *Three hours was lasted by the film.

(5)a. John has a new car.

b. *A new car is had by John.

Gazdar et al (1985) account for the ungrammaticality of examples like (2)b by assigning promise in examples like (2)a a semantic translation which prevents it undergoing the lexical rule that forms passive participles. One might try to account for the ungrammaticality of the other examples in the same way. It seems unlikely, however, that all cases where a sentence whose main verb takes an NP does not have a passive counterpart will allow such a semantic account.[1] It looks, then, as if we need a more restrictive metarule which does not apply to all VP rules that introduce an NP but only to a subset of them.

A more restrictive metarule is in fact proposed in Gazdar (1982). Gazdar distinguishes verbs which allow passivization from verbs which do not with a feature [TRN]. Utilizing this feature, he formulates the following metarule:[2]

(6) VP ---> V[TRN] NP W

⇓

VP[PAS] ---> V W (PP[by])

This is a metarule that derives PS rules from PS rules. As such, it is incompatible with current conceptions, according to which metarules derive ID rules from ID rules. Clearly, however, it could be reformulated so as to derive ID rules from ID rules. It is also incompatible, however, with a constraint on metarules that is proposed in Gazdar et al (1985). This is the constraint that no more than two terms, one of which is the variable W, can occur to the right of the ID arrow in the "pattern" (ie structural description) of the metarule. Clearly, if we want to maintain this constraint, we cannot simply reformulate (6).

It would be possible to formulate a more restrictive metarule that did not violate the proposed constraint if the NP in rules for VP's that have passive counterparts was distinguished by some feature from the NP in rules for VP's that do not have passive counterparts. We might call the feature OBJ. We could then formulate the following metarule:

(7) VP ---> W, NP[OBJ]

⇓

VP[PAS] ---> W, (PP[by])

This seems a fairly natural approach to the problem. In effect, however, it treats objecthood as a primitive notion. This might well be seen as an objection to it. Perhaps, however, it should be seen as a correct recognition that there is some truth in frameworks like relational grammar and lexical functional grammar, in which grammatical relations play a central role.

Even if the use of an OBJ feature is accepted as legitimate, (7) cannot be regarded as adequate. This is because there are sentences which have passive counterparts where it is doubtful whether the main verb should be analyzed as taking an NP. There are, for example, cases where the main verb takes a clause. The following illustrate:

(8)a. Everyone believes that John is a fool.

b. That John is a fool is believed by everyone.

The clauses in examples like (8)a have sometimes been analyzed as NP's. Such an analysis is assumed, for example, in Sag and Klein (1982). In Gazdar et al (1985), however, they are analyzed as bare clauses. At least two considerations favour such a treatment. Firstly the NP analysis is incompatible with a restrictive version of X-bar theory in which all rules are required to have a head on the right hand side. Secondly, it necessitates a feature to distinguish between NP's that exhaustively dominate clauses and ordinary NP's. As Cann (1983) notes, this seems rather undesirable. There are also cases where the main verb takes a PP. Consider, for example, the following:

(9)a. Everyone considers under the bed to be a good place to hide.

b. Under the bed is considered by everyone to be a good place to hide.

Again, one might assume that we are actually dealing with NP's. Again, however, X-bar considerations argue against such an analysis. Also relevant, as pointed out in Jaworska (1985), is the fact that there are other PP's in a typical NP position for which an NP analysis is very dubious. These are PP objects of a preposition. (10) illustrates:

(10) John appeared from behind the rock.

As Sag (1982) points out, such PP's cannot appear in initial position in wh-questions.

(11) *Behind which rock did John appear from?

This would be quite surprising if these PP's were NP's since of course NP objects of prepositions can appear in this position. It looks, then, as if these PP's should be analyzed as bare PP's. If they are, it seems natural to analyze the PP's in examples like (9)a as bare PP's as well.

It looks, then, as if there are sentences with passive counterparts where the main verb does not take an NP. We can provide for such sentences quite easily by extending the feature [OBJ] to the crucial constituents and replacing NP by XP in the metarule so that we have (12).

(12) VP ---> W, XP[OBJ]

⇓

VP[PAS] ---> W, (PP[by])

On the face of it, this is preferable to the metarule in (1).

We can turn now to the second of the two problems that arises for Gazdar et al's account of passives. This involves impersonal passives. As is well known, they occur in a variety of languages. A good example is Polish. Here, the active sentence in (13)a has both the personal passive counterpart in (13)b and the impersonal passive counterpart in (13)c.

(13)a. Wszyscy czytali tę książkę.
everyone read that(ACC) book(ACC)
'Everyone read that book.'

b. Ta książka była czytana przez wszystkich.
that(NOM) book(NOM) was read by everyone
'That book was read by everyone.'

c. Czytano tę książkę.
read that(ACC) book(ACC)
'People read that book.'

A rather different situation is illustrated in (14). Here, the active sentence has only an impersonal passive counterpart.

(14)a. Wszyscy wierzyli gazetom.
everyone believed papers(DAT)
'Everyone read the papers.'

b. *Gazety były wierzone przez wszystkich.
papers were believed by everyone
'The papers were believed by everyone.'

c. Wierzono gazetom.
believed papers(DAT)
'People believed the papers.'

There are grounds for saying that English too has impersonal passives. Rather like the data in (12) is the following data.

(15)a. Everyone who knew him believed that John would be back.

b. That John would be back was believed by everyone who knew him.

c. It was believed by everyone who knew him that John would be back.

One might suggest that (15)c should be viewed as an 'extraposed' counterpart of (15)b. However, this seems dubious because, as a number of people have noted, there are verbs which can appear in sentences like (15)c but not in sentences like (15)b. The following illustrate:

(16)a. Everyone who knew him felt that John would be back.

b. *That John would be back was felt by everyone who knew him.

c. It was felt by everyone who knew him that John would be back.

Moreover, as Marantz (1984) has pointed out, sentences like (15)c and (16)c are unlike unquestionable instances of extraposition in that the complementizers can sometimes be omitted and the clauses are not islands. Thus, we have a contrast between the examples in (17) and those in (18) and (19).

(17)a. *It stinks John should do that.

b. *What does it stink that John should do?

(18)a. It was believed John would do anything.

b. What was it believed that John would do?

(19)a. It was felt John would do anything.

b. What was it felt that John would do?

It seems, then, that there is a quite strong case for analyzing (15)c and (16)c as impersonal passives.

Within GPSG, the obvious way to accommodate impersonal passives is to formulate an additional metarule. What sort of metarule would be appropriate for Polish is unclear to me. For English, however, one might suggest the following:[3]

(20) VP ---> W, S'

$\Downarrow$

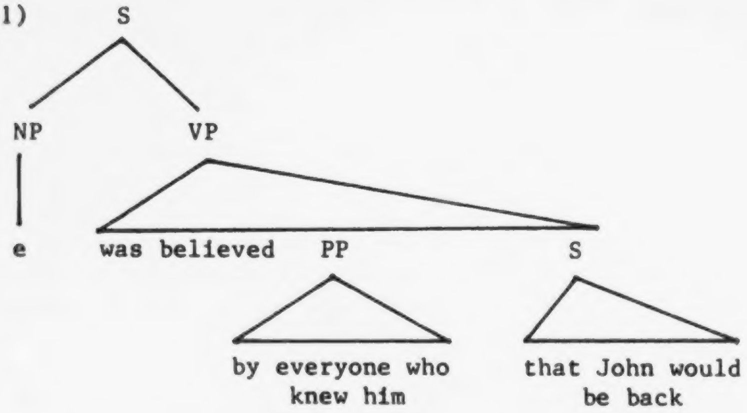
VP[PAS, it] ---> W, S', (PP[by])

One would also need an additional lexical rule to provide a set of passive participles with appropriate semantic translations to appear in impersonal passives. I will not attempt to formulate such a rule, but I assume there is no difficulty in principle here.

It is clear, then, that we can provide an analysis of impersonal passives within GPSG. There is, however, a serious objection to the analysis. If we provide for impersonal passives with an additional metarule and an additional lexical rule, we are in effect claiming that it is accidental that passive participles appear in both personal and impersonal structures. The variety of languages which have both personal and impersonal passives suggests rather strongly that this is not the case. It seems desirable, then, to analyze personal and impersonal passives as the reflection of a single rule or principle. As far as I can see, however, there is no way to do this within GPSG.

Interestingly, there seems to be no problem here for GB. For GB, personal passives involve the movement of a constituent which requires case from a position to which no case is assigned into a subject position to which no theta role is assigned. It is crucial that no theta role should be assigned to the subject position since otherwise there would be a violation of the theta criterion, which requires an argument to have one and only one theta role. Impersonal passives will involve the same D-structure as personal passives but will involve no movement into subject position. Instead, a dummy will be inserted. Thus, both (15)b and (15)c will derive from the following D-structure, (15)b through movement, and (15)c through insertion of a dummy.

(21)



As Jaworska (1985) shows, it is possible within this approach to provide for passive participles with a single lexical rule. The rule will remove the ability to assign a theta role to subject position and the ability to assign case. Where the basic verb has a complement that requires case, the complement will have to move into subject position.[4] Impersonal passives will arise if some verbs that undergo this rule do not include among their complements a constituent which requires case. In this situation, no movement will be necessary. In the case of *believe*, we can assume that the clause is optionally marked as requiring case, so that movement may or may not be necessary. In the case of *feel*, on the other hand, the clause will never be marked as requiring case, so movement will never be necessary.[5] On this account, it is no accident that passive participles appear in both personal and impersonal structures since they arise through the same rule in both cases. On the face of it, then, GB is more satisfactory than GPSG here.

FOOTNOTES

* I am grateful to Gerald Gazdar and Ewa Jaworska for helpful comments on this paper. Its failings are, of course, my responsibility.

1. As Wasow (1980) points out, a semantic account seems particularly unlikely in the case of examples like (5)b given the grammaticality of examples like (i):
   (i) A new car is owned by John.

2. A TRN feature is also exploited in Cann's (1983) analysis of Latin passives.

3. A metarule for Latin impersonal passives is formulated in Cann (1983).

4. Jaworska identifies constituents as requiring case by assuming a feature CASE which has as one of its values ZERO and reformulating the Case filter as a ban on constituents with the feature specification [CASE, ZERO] at S-structure. Within this approach, case marking conventions are rules that change the value of the CASE feature from ZERO to some other value (NOM, ACC, etc).

5. A question arises as to why movement is not possible in cases where it is not necessary. Within the approach of the preceding footnote, this could be attributed to a requirement that the moved element must have all the feature specifications of the landing site. Subject position will have the feature specification [CASE, ZERO]. Hence it will only be possible to move constituents with this specification into subject position.

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REDUPLICATED CONSTRUCTIONS IN CHINESE AND QUESTIONS OF GENERATIVE POWER*

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1. Introduction

In the following pages, it is my intention to demonstrate the problems that some data from Mandarin Chinese pose for modern linguistic theories. The study focuses on the reduplication involved in a construction known as the 'A-not-A' construction and I shall not try to provide a grammar for a large fragment of Chinese. Instead, this paper will use the data and possible descriptions thereof to investigate the question of whether context-free languages, hereinafter CFL's, are sufficiently adequate for the task of describing certain linguistic phenomena. Taking Pullum (1984) as a starting point, I shall consider whether the Chinese data in question constitute a case against the claims of Generalized Phrase Structure Grammar (hereinafter GPSG).

In the rest of this introduction, I offer some methodological justification for dealing with this problem. I then proceed to an overview of the A-not-A construction. Section 3 examines methods that may be used to disprove claims of non-context-freeness, and their applicability to the Chinese data.

1.1 Context-freeness and the Methodology of GPSG

The fall and rise of context-free phrase structure grammars within linguistics has been well chronicled, (Gazdar (1982), Pullum and Gazdar (1982)), and I do not intend to repeat history here. Rather, I should like to comment briefly on the status of the question 'Is English a CFL?'

It has been assumed by many linguists in opposing camps that the framework of GPSG presupposes a positive answer to this question. Their view is that a demonstration of the non-context-freeness of natural languages constitutes a knock-down argument to the central claims and methodological tenets of GPSG. This is obviously the thrust of Higginbotham (1984), for example.

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In fact, the paradigm of GPSG has always recognised the possibility of a demonstration that English, or some other natural language, could not be described by a context-free phrase structure grammar (hereinafter CFPSG), (cf Gazdar (1982:177) and more recently Pullum (1984)).

The current answer that GPSG offers to the question of whether English is a CFL is a provisional affirmative; none of the arguments claiming to prove the non-context-freeness of English have any substance (Pullum and Gazdar (1982)), although there may be other natural languages whose description does require supra-context-free power, (Pullum (1984), and below). Given this possibility, an important methodological advantage of GPSG is that future lines of research are, in a sense, predetermined; the constrainedness of the theory allows a very clear statement of the nature and status of counter-examples and the generative capacity of the theoretical apparatus may always be revised upwards, provided a proof is given of the necessity of such a change, along with good motivation for the proposed extension. In contrast with the various models of transformational grammar available, we may know that a particular grammatical description is the least extravagant possible. Viewed in the above terms, this study is, along with Pullum (1984), an inquiry into the limits of context-free description.

It is worth emphasising at this point that, during the course of this paper, we shall be concerned with weak, rather than strong, generative capacity. That's to say that we shall be interested in showing that it is technically feasible to give a context-free grammar for the constructions in question, not that the resultant grammars are the most linguistically satisfying. As Pullum and Gazdar note (1982:498), linguistic theory demands that we pay attention to the question of strong generative capacity, but in this paper I shall merely point to ways in which the less attractive consequences of opting for context-free description may be avoided.

2. Data on the A-not-A question

2.1 Preliminaries

Before launching into a description of the data, the following notes are in order.

First of all, the collection of consistent data for any aspect of Chinese linguistics is very difficult. In this essay, I have tried to arrive at grammaticality judgements relative to standard Mainland Chinese, known in the People's Republic of China as putonghua, and in the West as Mandarin. The situation is however complicated by several factors. Available modern work in linguistics typically takes as standard Mandarin as spoken in Taiwan (kuoyu). Even within this literature, there are many contradictory judgements. Putonghua, in spite of considerable efforts at language reform, also contains considerable variation; standardization usually focusses on phonological, lexical and major syntactic features, rather than on the finer points of syntax. Variation among the judgements of my informants was in some cases so great as to leave no discernible pattern.

Secondly, the usage of the word `Chinese' later in this essay, might be deemed by some to be a little loose, as all my discussion, unless otherwise stated, refers to Mandarin Chinese alone. This looseness may be justified by the fact that A-not-A constructions exist in Cantonese (cf Chao (1976:198)), and forms of sentential reduplication may be found in other dialects. For example, Zhang (1979) claims that the Chaoyang dialect of the Southern Min dialect group has reduplication of whole sentences for hyperbolic effect.

Thirdly, some general typological notes. Mandarin Chinese is canonically SVO (Li and Thompson (1981:23ff)). There are many constructions which do not adhere to this order, but they will only be of marginal concern to us here. VO is typically assumed to form a verb phrase (Li and Thompson (1981:139ff)). There is no inflectional morphology.

2.2 Why might the A-not-A construction be a problem?

One option for the speaker of Mandarin, if he wishes to ask a yes/no question, is to use the A-not-A construction (following the terminology of Rand (1969), Fenn and Tewksbury (1967) and most other sources). Li and Thompson's (1981) discussion of the construction occupies about a quarter of all the space they devote to questions; that is, it is not a marginal phenomenon.[1]

In the following section, I give a characterisation, in terms as neutral as possible, of the A-not-A construction, and potentially related constructions.

2.2.1 The simplest cases

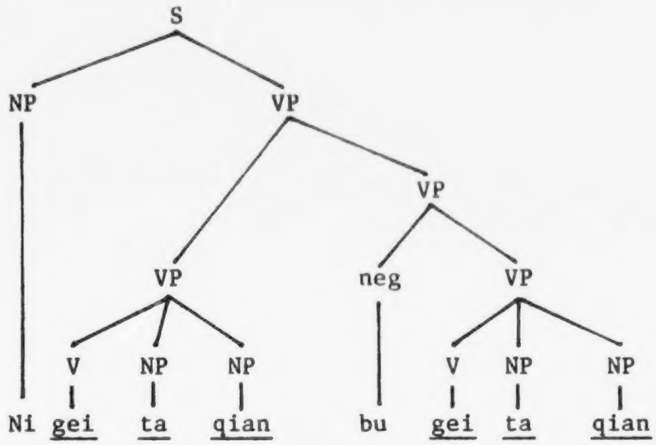
In informal terms the construction involves a verb phrase and the negated repetition of that verb phrase, as the following illustrates:

(1) Ta qu bu qu?
he go not go
`Is he going?'

(In example sentences, reduplicated sections are underlined.) The structure appears simple enough, when limited to sentences like that above, but there are numerous complications in the behaviour of sentences involving aspectual marking or reduction in either verb phrase. A more complicated example is (2), the tree structure for which is given in (3).

(2) Ni gei ta qian bu gei ta qian?
you give him money not give him money?
'Do you give him money?'

(3)



Obviously, there are various details being glossed over here, for example the precise syntactic representation of the negation.

It should by now be clear why the A-not-A construction is so called; questions are formed by negating the repetition of some string A. We may also see why it is that such a construction might be problematical for certain linguistic theories; such constructions look very much like the **WW** languages, where **W** is a variable over strings of symbols, as described by, for instance, Aho and Ullman (1972:198). This source contains a proof that such languages may not be generated by a grammar using only context-free resources. Huang (1982b:281) has explicitly claimed that the same holds true for the Chinese construction under discussion. Let us consider the reasons for making such a statement, for which purpose we need to recall the definition of a context-free grammar. The following is taken from Hopcroft and Ullman (1979:79ff).

(4) A context-free grammar is a quadruple, V, T, P, S, where

(i) V is the finite set of variables (or syntactic categories),

(ii) T is the finite set of terminal symbols (or lexical items),

(iii) P is a set of rules which rewrite single elements of V as strings of symbols drawn from V and T, and

(iv) S is the ‘start symbol’, ie the syntactic category that appears at the highest node, or root, of a tree.

It is easy to demonstrate that grammars designed along the above lines may not generate a set of trees with reduplicated structure, as illustrated by the Chinese example above. An informal exposition may be based upon the following set of rules.

(5) (i) S --> NP VP
(ii) VP --> V NP NP
(iii) VP --> VP bu VP
(iv) NP --> ni ‘you’
(v) NP --> ta ‘he’ or ‘him’
(vi) NP --> qian ‘money’
(vii) V --> gei ‘give’

While it is obvious that the sentence in (3) may be generated by the above grammar, it should be equally obvious that this grammar will generate other sentences without reduplication, such as (6).

(6) Ni gei qian ta bu gei ta ni
you give money him not give him you
‘You give him to money (and) don’t give yourself to him’

We may also note that the only reason why we succeed in reduplicating the verb *gei* in these cases is that there is no other choice. If we were to introduce another verb, then even this consistency would be lost. The above example allows an intuitive grasp of the problem that reduplication presents for context-free grammars. Before examining ways of circumventing this problem, let us take a further look at some relevant data.

2.2.2 Reduced forms

The sentences in (7) below represent a brief illustration of the possibilities of reduction. The unreduced form of the sentence in question appears on the line marked (iii) below, with the words in the reduced versions of the sentence aligned above their counterparts in the unreduced sentence. (i) and (ii) contain examples of possible reductions on the left-hand side and right-hand side (hereinafter LHS and RHS respectively). (8) is a possible tree structure for the unreduced sentence, (assuming that modals are dominated by a projection of V, cf Gazdar, Pullum and Sag (1982)).

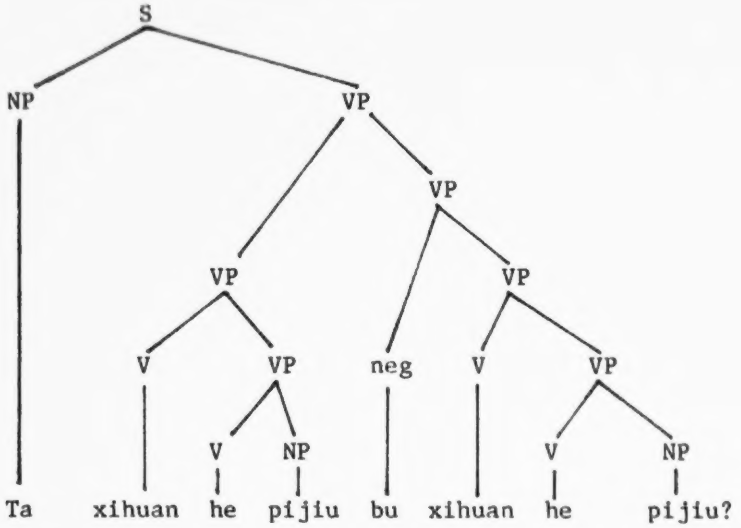
(7) (i) Ta xi bu xihuan he pijiu?
Ta xihuan bu xihuan he pijiu?

(ii) Ta xihuan he pijiu bu?
Ta xihuan he pijiu bu xihuan?
Ta xihuan he pijiu bu xihuan he?

(iii)	Ta	xihuan	he	pijiu	bu	xihuan	he	pijiu?
	NP	modal	V	NP	neg	modal	V	NP
	he	like	drink	beer	not	like	drink	beer

'Does he like to drink beer?'

(8)



Note that all of the sentences given in (7) are grammatical, the first example in (7i) containing the usually bound morpheme xi.[2] Chao claims that this reduced form of the A-not-A question, involving what he terms 'ionised' morphemes, is a recent borrowing from Cantonese, (Chao (1976:198)). Such forms are common within this type of A-not-A questions; one informant deemed grammatical examples with 10 randomly chosen bisyllabic verbs. Such sentences are, however, completely ungrammatical when the reduction occurs on the opposite side, viz:

(9) *Ta xihuan he pijiu bu xi

The first sentence in (7ii) above illustrates the case where reduction of the RHS proceeds so far as to leave only the negation marker, a construction typical of the northern dialects, especially of colloquial Pekinese. The third sentence in (7ii) is one of the cases in which a reduction leaves more than the left-most constituent (or first syllable thereof). The inconsistencies in the data, alluded to in section 2.1, were found to be greatest in respect of sentences of this type. While some generalisations can be made over the data obtained (Calder (1984)), it is impossible to be sure that these are not the results of some random factor. Given that the consensus within the literature and among my informants agrees on the grammaticality of unreduced and, what I shall call, fully reduced forms, I shall concentrate on these sentence types and not discuss, in any detail, the intermediate, semi-reduced forms. Neither shall I investigate the complex paradigms created by the interaction of the A-not-A phenomenon with the behaviour of aspect markers under negation.

2.2.3 What can substitute for A?

A more important concern for us than the description of the aspectually marked paradigms is the need to find some principled specification of the class of categories that may enter into A-not-A constructions. It is obvious, from (5) above, that the construction may consist of full VP's, but we need to know the categorial identity of the elements that appear in the reduced forms. In cases where a single word alone is repeated then the elements involved fall into the (English) classes of preposition (9)[3], adverb (11) and degree modifier (12), as well as into the classes of verb and modal verb (13) (there is a lot of lexical idiosyncrasy in the first two cases).

(10) PREPOSITION
Ni gei bu gei ta mai shu
NP P neg P NP V NP
you give not give him buy books
`do you buy books for him?'

(11) ADVERB
Ta zai bu zai ti zuqiu
NP ADV neg ADV V NP
he again not again play football
`Is he going to play football again?'

(12) DEGREE MODIFIER
Zhei ben shu tai bu tai zhong?
DET MW N DM neg DM ADJ
this MW book too not too heavy
`Is this book too heavy?'

(13) MODAL
Wang Xiansheng neng bu neng gei wo jie qian
PN Title modal neg modal P NP V NP
Wang Mr. able not able give me lend money
`Is Mr. Wang able to lend me money?'

There appears to be an absolute proscription on reduction in sentences containing reduplicated adverbs. Thus, although the sentence

(14) Ta manmarde pao bu manmarde pao?
he slowly eat not slowly eat
'Does he eat slowly?'

is considered grammatical by Li and Thompson (1981:538, example 83), any reduction is ungrammatical.

2.3 Other constructions involving reduplication

We may note other constructions that seem to require repetition of lexical material.[4] Obviously, if we decide to invoke any syntactic mechanism to ensure that the lexical material in A-not-A questions is faithfully reduplicated, this may also be of use in accounting for these constructions. Likewise we will want any analysis of the A-not-A construction to be readily extendable to these constructions.

For instance, durative constructions with objects are formed by a verb phrase consisting of verb plus object, then the repeated verb followed by a noun phrase describing the length of time over which the action occurred. Thus (15), for which a possible structure is (16).

(15) Lao Deng xue Yingwen le, xue san nian le
Old Deng study English ASP, study three year ASP
'Deng has been studying English for three years'

(16) $[_{S}[_{NP}$ Lao Deng]

$[_{VP}[_{VP}$xue $[_{NP}$Yingwen] le]$[_{VP}$xue $[_{NP}$san nian] le]]]

Very similar to such durative forms are the 'resultative complement' forms (cf Huang (1983), Tewksbury and Fenn (1967:130)). Again, if the verb takes an object and a resultative complement, then the verb must be repeated, first preceding the object, then preceding the resultative complement, as in:

(17) Ta zou lu zou de hen lei
He walk street walk till very tired
'He tired himself out walking'

Such sentences are discussed in greater detail in section 3.2.1. below.

2.4 Comparison with VP coordination

One possible analysis of the A-not-A construction is to view it as a special case of VP coordination; Wang (1964, 1965) offers precisely such a treatment. The following is a brief sketch of potentially relevant facts.

VP coordination is very common in Chinese, both as a result of syntactic processes, and as a discourse phenomenon when subject pronouns are dropped. Typical examples of the former are the durative and resultative complement constructions, described above. The latter do not concern us particularly here. An example of ordinary verb phrase coordination is (18):

(18) Lao Wang dao menkour, bu neng jin qu
Old Wang arrive door, not able enter go
'Wang got to the door and/but couldn't go in'

There is a very wide range of coordinating and subordinating conjunctions in Chinese. The most common of the former are *ye*, with the conjunctive meaning 'also', which can appear before both conjuncts and *haishi*, with the disjunctive meaning 'or', which typically appears only before the second conjunct. These are illustrated in (19) and (20) respectively.

(19) Zai Beijing de shihou, wo ye zuo shi ye
At Peking RM time, I also do business also
waryiwar.
play
'When I'm in Peking, I do business and have some fun'

(20) You kongr de shihou, wo kan shu haishi ting
Have free-time RM time I read book or listen
yinyue.
music
'When I have free time, I read or listen to music'

As illustrated by example (18) above, there is no requirement that an overt conjunction appear. If there is no overt conjunction, then the interpretation of the coordinated verb phrase is highly context-dependent. Thus the sentence:

(21) Ta chi fan chi mian
he eat rice eat noodles

may have either a conjunctive reading, 'he eats rice and noodles', or a disjunctive, interrogative meaning, 'does he eat rice or noodles?'

The most important difference between A-not-A questions and VP conjunction is that the reduction phenomena associated with the former do not occur in the latter. Informally, in the cases of left-hand side reduction in A-not-A questions, eg (22):

(22) Ni *he* bu *he* pijiu
you drink not drink beer
'Do you drink beer?'

we might wish to say that the noun *pijiu* is construed as the object of both occurrences of the verb *he*. A similar interpretation of a sentence with conjoined, *non-identical* Vs cannot use the same syntactic structure. Thus (23) is deemed ungrammatical with *pijiu* construed as the object of both *niangzao*, and *he*.

(23) *Ta niangzao bu he pijiu
he brew not drink beer
'He brews but doesn't drink beer'

(24) a) (Guanyu) pijiu, ta niangzao bu he
as for beer he brew not drink

b) Ta niangzao pijiu, bu he
he brew beer not drink

The reading discussed above is available if the noun in question is topicalised ((24a) above) or appears as the object of the first verb (24b), although the first rendering is preferred.

However, more than a little care must be taken here, as some verb coordination might seem to be possible, provided there is an overt conjunction: Li and Thompson (1981, 18.3.1 ex.57) offer the sentence:

(25) Ni chao haishi zheng zhei ge qingcai
you fry or steam this MW vegetable
'Do you fry or steam this vegetable?'

This example might be excluded from the data on the grounds of dialect difference, as Li is a native of Taiwan. However the situation is more complex than this; Huang (1982b:278-279), also from Taiwan, claims that 'coordinate V's' are 'quite unnatural', and stars the following sentences:

(26) *Zhangsan zhong (ye) mai gua
Zhangsan grow (too) sell melons[5]
'Does Zhangsan grow or sell melons?'

(27) *Zhangsan xie (haishi) mai shu
Zhangsan write (or) sell books
'Does Zhangsan write or sell books?'

My informants agreed with Huang's grammaticality judgements in the cases where there is no overt conjunction; one of them agreed with Li and Thompson in respect of sentences with overt conjunction. In what follows, I shall follow Huang in the case of the former, and Li and Thompson in the case of the latter.

Another respect in which the A-not-A construction is claimed to differ from ordinary verb phrase coordination is in the non-reversibility of constituents. Chao (1968:267-

270) notes that `the order of items in coordination is grammatically reversible [...] but in the particular case of V-not-V [= A-not-A JC] questions the order is fixed ...'.

Thus Chao would rule out sentences such as (28):

(28) *Ni bu lai lai
you not come come
`Are you coming?'

Chao notes that this constraint does not apply in cases where there is an overt conjunction.

From the above discussion, it is fair to conclude that, in some dialects at least, certain verbal constructions are available only in A-not-A questions, and therefore when there is identity of lexical material. I shall return to this point in section 3.2.2, dealing with the adequacy of phrase structure approaches, after a formal statement of the descriptive problems posed by A-not-A questions and some discussion of ways to obviate these problems.

3. How should we deal with reduplication?

Pullum (1984) points out the major problems that the phenomenon of reduplication causes to grammatical formalisms which use only context-free resources. The essential nature of the difficulty is that a construction of the form

(29) $a_1\ a_2\ a_3 \ldots\ a_n\ b_2\ b_3 \ldots\ b_n$

requires supra-context-free devices for its generation, under the following conditions:

i) a_i and b_i are words or morphemes of a particular language

ii) there is some sort of dependency between a_i and b_i, and

iii) there is no upper bound on n.

In the following sections I shall first of all review what arguments may be used to demonstrate that the existence of such constructions does not necessarily compromise the thesis of the context-freeness of natural language, and then consider how the A-not-A construction may be handled within the syntactic framework of GPSG.

3.1 Dealing with apparent non-context-freeness

In this section I shall look at two of the ways in which GPSG may avoid invoking non-context-free syntactic apparatus, while describing putatively non-context-free constructions. I shall avoid discussion of the more well-known aspects of GPSG, such as slash categories or the extensive use of features, assuming familiarity with the systems as introduced by Gazdar and others (e g Gazdar (1982), Gazdar and Pullum (1982), Gazdar, Klein, Pullum and Sag (1985) etc). Before examining the techniques that will help us in this, we should first of all consider the importance of the term 'stringset' in this discussion.

The notion of the stringset is used heavily by Pullum and Gazdar (1982) in their debunking of the published arguments claiming to demonstrate that descriptions of natural languages must invoke greater than context-free power. It involves treating natural languages 'purely as sets of strings of words' (op. cit. p471); in other words, the object of interest is the weak generative capacity of the apparatus required. The notion is crucially implicated, as the authors spell out, in their treatment of Dutch cross-serial dependencies (op. cit. p489), a construction which is putatively non-context-free (cf. Huybregts (1976), Bresnan, Kaplan, Peters and Zaenen (1982)). As they say, their context-free approach may not generate a description that will feed a compositional semantics. Nevertheless, the grammar they provide does succeed in generating the required sentences and does not overgenerate.

3.1.1 Expanding the data

One type of argument that may be invoked to avoid increasing the power of a grammar is that of increasing the data set that the grammar accounts for. Pullum and Gazdar (1982:490ff) use this approach to deal with what had been claimed by Postal to be a construction intractable for context-free methods. Postal's claim was that the process of 'noun incorporation' in Mohawk required a string-copying operation. Langendoen, in his reconstructed version of the argument (ibid. for references), intersects a finite state language with some example Mohawk sentences, and shows that the resulting language is of the **WW** type, as described in section 2.2.1 above, and therefore not a context-free language. This implies that the language with which the finite state language was intersected cannot be context-free.

The argument is sound, but Pullum and Gazdar note that the same may perhaps not be claimed of the initial premises. In particular, they contest the claim that the nouns involved in incorporation must be identical. They give data showing that in fact there are similar sentences on which the requirement for identity does not hold. Therefore, we may allow our grammar to generate sentences with any noun substituting for the category symbols dominating the positions in question. In certain cases the same noun may be substituted for both positions, giving rise to those constructions that are thought to be context-sensitive. However we do not need to use context-sensitive apparatus to check that identity of insertion has occurred, as any non-identical insertions will be grammatical sentences of the other type of construction. In other words, we deny the first condition in section 3 for non-context-freeness, by claiming that there is no dependency between the morphemes in question. We shall revisit this particular strategy in looking at A-not-A questions in the GPSG framework.

3.1.2 Semantic filtering

Pullum (1984) gives examples showing that, if the semantic component of a grammar is allowed to rule out certain of the strings produced by a CFPSG, then the set of strings sanctioned by the grammar as a whole may be undescribable using simple context-free rules. His particular example uses a context-free syntax and filtering to turn a context-free stringset into a **WW** language. As with the above ploy, it will be of use in our descriptions of the A-not-A construction, and we shall return to it in section 3.2.3. It is obvious, but worth emphasising, that any argument invoking semantic filtering is worthless unless it makes explicit the means by which such filtering takes place.

The method used by Pullum is to associate a particular semantic type with each terminal symbol. Once a sentence has been generated, its semantic structure is built up, in accordance with the semantic specification for each rule: this much is standard GPSG. It may happen, however, that at some point the semantic rule calls for semantical objects of inconsistent types to be combined. In this case, no semantic interpretation of the sentence may be obtained, and the sentence is therefore not accepted by the grammar.

3.2 Chinese within the GPSG framework

I shall now turn to possible treatments of the Chinese data within the framework of GPSG. Armed with the techniques described above, we shall look first at some suggestions made by Huang (1983) in respect of the resultative complement construction, and then consider how we might approach other potentially troublesome constructions involving reduplication.

3.2.1 Resultative complements

Chu-Ren Huang (Huang 1983) offers phrase structure rules in the immediate dominance and linear precedence (hereinafter ID/LP) format of Gazdar and Pullum (1982:18f) to deal with the resultative form of verb reduplication described in section 2.4. above. He contests the assertion made by C-T James Huang, during a lecture to the Linguistic

Institute, 1983, that such sentences require more than context-free power for their generation. Thus, sentences of the form

(30) Ta [$_{VP}$ qi ma] [$_{VP}$ qi de hen lei]
he ride horse ride till very tired
'He rode the horse until he was very tired'

where there is repetition of lexical material, may be assimilated to those of the form

(31) Wu da niang [$_{VP}$ shang jie] [$_{VP}$ zou de hen lei]
Wu big wife up street walk till very tired
'Eldest Wu's wife got very tired by walking downtown'

(Glosses and translations are from Huang (1983:11)). Huang claims that these are instances of a general schema of verb phrase conjunction, of which the immediate dominance (ID) statement is:

(32) VP --> VP, VP
[+VN] [+COMPL]

where the different features are required to allow for the occurrence of de in the second VP. The feature [VN] forces the expansion of the VP to contain an object. The accompanying linear precedence (LP) rule is:

(33) VP < VP
[+VN] [+COMPL]

As Huang mentions, this is a very specialised LP rule and one which it is hard to motivate with respect of the rest of the grammar. (We may note however that it will be of use in dealing with the durative constructions of section 2.3.) Thus the formal argument is identical to that alluded to in section 3.1.1 above, namely we deny the thesis that there is a dependency between the constituents in question.

In order to facilitate later discussion, I repeat in (34) to (39) below some of Huang's rules. As mentioned above, these are given in the ID/LP format, due to Gazdar and Pullum (1982). For our purposes, the most important of these are the rules for the expansion of S, and VP, which are given below.

(34) a. S --> XP[PRED]

b. [$_{VP}$ xia yu le]
fall rain ASP
'It is raining'

(35) a. S --> NP, XP[PRED]

b. [$_{NP}$ hua] [$_{AP}$ hen mei]
flower very beautiful
'Flowers are very beautiful'

(36) a. VP --> V, NP

b. [$_{V}$ da] [$_{NP}$ ta]
hit him
'hit him'

(37) a. VP --> V

b. [$_{V}$ fei]
fly
'fly'

We also need the following linear precedence statements:

(38) a. V < NP < AP < N
b. NP < VP

and the following Feature Co-occurrence Restriction:

(39) [PRED] $\supset$ [+V]

which guarantees that expansions of S have as their predicate either verb or adjective phrases.

With Huang's rules as our basis, let us look at the potential for context-free descriptions of other examples of reduplication.

3.2.2 Context-free treatments of reduced A-not-A questions

Given the data in section 2, we may now see how various attempts to show that Chinese is not a context-free language may be defused. I shall first of all consider the fully-reduced forms of A-not-A questions, i e those of which one side has been reduced to a single lexical or sub-lexical element. I shall then investigate the implications of the full forms of A-not-A questions for the power of our theoretical devices.

Consider first of all the fully reduced forms of A-not-A questions. In these cases one might suppose that a proof of the following form demonstrates the non-context-free nature of this construction. (James Huang (1982b:281) suggests this possibility, but does not provide any formal proof. The following is one possible elaboration of his comments.)

Verb conjunction in Chinese is allowed exceptionally in the case of A-not-A questions, hence the grammaticality judgements for examples (23) section 2.4, repeated below:

(40) a) Ta niangzao bu niangzao pijiu?
he brew not brew beer
'Does he brew beer?'

b) *Ta niangzao bu he pijiu.
he brew not drink beer
'He brews but doesn't drink beer'

c) NP V_i neg V_j NP

Assuming a representation like c), we must invoke a rule introducing V_i and V_j which imposes an identity condition on the rewriting of these elements; such a rule is obviously context-sensitive and so this construction may not be generated using only context-free resources.

The flaw in this argument is the assumption that the identity condition can not be expressed in the rule itself, which would only be the case if the class of items introduceable by V_i and V_j was infinite. (This would contravene the definition of context-free grammar, cf (4) i) above.) However, calculations suggest that there is a finite number of phonologically and syntactically distinct verbs in Chinese,[6] and we may use this information to demonstrate that certain forms of the A-not-A question are tractable within a context-free framework. Thus if we assume a rule of the form

(41) $V_i \longrightarrow V_i$ bu V_i

where V_i stands for that preterminal category that may be rewritten only as niangzao, then we may use rules of this form to generate all and only the legal conjunctions of Chinese.[7] In the cases where the verb is repeated to the right of the full VP, we may borrow Chu-Ren Huang's tactic of assimilating such forms to ordinary verb phrase coordination, in which case there is no identity requirement, as example (24b) shows, repeated here:

(42) Ta niangzao pijiu, bu he.
he brew beer not drink
'He brews, but doesn't drink beer'

Thus, James Huang's assertion that '[constructions having lexical or smaller-than-lexical categories as conjuncts] must be generated by context-sensitive rules' (1982b:281) is completely groundless.[8]

3.2.3 Unreduced forms

Having seen that reduced forms of the construction may be successfully handled by context-free rules, let us turn to consideration of the unreduced forms, i e those consisting of whole VP's. The obvious course of action is to assimilate A-not-A questions to ordinary verb coordination, again adopting Chu-Ren Huang's arguments to demonstrate that no identity condition need be stated. This would be fine, were it not for Chao's observation (1968:269) on the non-reversibility of A-not-A question forms (see section 2.4 above). The import of this is that, while sentences of the form X-not-X, X-not-Y, and Y-not-X (where $X \neq Y$) are all permissible, sentences of the form not-X-X are not. This opens up the possibility of a demonstration that Chinese is a non-context-free language, the intuitive basis for which is as follows. If we have a rule for VP coordination that does not specify that the right-hand conjunct is to be negated, then the grammar overgenerates producing sentences of the form not-X-X; on the other hand, if we do impose such a condition, then we may not generate the grammatical sentences of the form not-X-Y. In formal terms we may describe the situation thus. Let L be the set of grammatical Chinese sentences, and L_{cf} be that set of sentences generated by free interaction of VP conjunction and negation. L differs from L_{cf} in containing sentences of the type not-X-X. Let L_{cs} be the set of these sentences defining a language which is provably context-sensitive. We now need to know the formal properties of the relative complement of L_{cf} and L_{cs}. At present the result of this operation is unknown (Hopcroft and Ullman, (1969:132)), and I shall therefore assume that the result in question is not provably context-free.

The above demonstration requires a lemma to the effect that the sentences involved may be infinitely long, otherwise we could invoke similar argument to those used in the VP conjunction case above. In other words, we should need to know that A-not-A questions may consist of reduplicated VPs of unbounded length.

However, demonstrating the required premise is not so easy to do. The key to the techniques that might be of help is the concept of recursion. If we can show that, for example, it is possible to have a relative clause in the object NP of an A-not-A question, then we may use the argument, due to Miller and Chomsky (1963:470ff), that any bound on the depth of recursion is arbitrary, and a matter of psychological, rather than linguistic, theory. However, sentences involving recursion were not well received by my informants. Let us look first at the structure of Chinese relative clauses.

Chinese relative clauses are pre-head, consisting of an embedded sentence, followed by the relative marker de.

Thus, the 'John loves Mary' of Chinese relative clauses:

(43) Ni renshi nei ge $[_S[_{VP}$ dai yanjing] de] xuesheng
You know that MW wear glasses DE student
'You know the student wearing glasses'

Noun phrases may be modified by several relative clauses, as in (44):

(44) Ni renshi nei ge $[_S$ dai yanjing de],
You know that MW wear glasses DE

$[_S$ bijiao gao de] xuesheng
rather tall DE student

'You know the rather tall student wearing glasses'

However, A-not-A questions involving this sort of recursion were very badly received, and even one level of embedding led to judgements of ungrammaticality, as in (45):

(45) *Ni renshi neige dai yanjing de xuesheng
You know that wear glasses DE student

bu renshi neige dai yanjing de xuesheng?

'Do you know the student wearing glasses?'

Similar reactions were found to A-not-A sentences involving adjectival modification; usually adjectival modification takes the same syntactic form as the relative clauses described above. Thus (46)i) is unacceptable, unlike ii):

(46) i) *Biaozhun fayin hen kequ

ii) Biaozhun de fayin hen kequ
standard DE pronunciation very desirable

'Standard pronunciation is very desirable'

There is a small class of adjectives that may modify nouns directly, but even these were found to be ungrammatical in A-not-A question forms. Hence:

(47) *Ni you jiu shu meiyou jiu shu
you have old book not-have old book
'Do you have old books?'

The above sentences obviously do not provide the basis for a proof of the unbounded length of the VP's involved in A-not-A questions. One further possible source of recursion is the embedding of sentential complements. The sentences in (48) might be claimed to be such examples:

(48) a) Ni renwei wo ben bu renwei wo ben?
you think I/me stupid not think I/me stupid
'Do you think I'm stupid?'

b) Lao Li rang ta qu Shanghai bu
Old Li let he/him go Shanghai not

rang ta qu Shanghai?
let he/him go Shanghai

'Does Li allow him to go to Shanghai?'

Neither of these sentences was well liked by my informants. However, only sentence b) was reckoned ungrammatical, and only then by one informant. One might want to claim that this does not in fact vitiate the claim that, as a stringset, Chinese is context-free, as one may argue that we do not need to analyse such constructions as containing embedded sentences, but rather adopt an approach which assigns the following structure to the reduplicated portions of the above strings:

(49) $[_{VP}$ V NP VP $]$

This treats rang as the head of the construction, which might not be desirable, if we wish to assimilate it to prepositions. On the other hand, given that varying patterns of control phenomena are found with particular coverbs, we might well wish to have individual rules of the above kind for introducing them (in the manner of Gazdar (1982:148ff)).[9]

The grammaticality of other sentences which might be analysed as having embedded sentential complements has not been tested in this study. A relevant example is:

(50) Ta shuo $[_S$ ta shi Meiguo ren$]$ bu
he say he COP American man not

shuo $[_S$ ta shi Meiguo ren$]$
say he COP American man

'Does he say he's American?'

My intuition is that this sentence is as problematic as examples (45) and (47) above, although this of course requires verification from a native speaker. If it were found to be the case that such examples are wholly ungrammatical, then we should have no justification for accepting as a premise the idealisation that reduplicated strings in Chinese may be infinitely long or for accepting the thesis that these cases of reduplication necessarily implicate grammars of greater than context-free power. It should be noted that the elimination of recursion from certain portions of the grammar is easy to achieve; the addition of a feature may prevent a rule that introduces a recursive element from applying.

On the other hand, if a good case can be made for such constructions being potentially unbounded, then this is the point at which it makes sense to invoke Pullum's notion of semantic filtering (Pullum (1984) and section 3.1.2 above). In this case, however, the constraint that forbids a certain set of strings and perhaps renders the stringset non-context-free is presumably more pragmatic than semantic, as it will need to explain how X-not-X is readily interpretable as a question, while not-X-X is necessarily contradictory.

4. Conclusions

The preceding discussions have looked in depth at the problems that the phenomenon of reduplication can cause for linguistic formalisms with restricted expressive power. We have seen under what assumptions the A-not-A construction in Chinese may be dealt with by such formalisms. In particular, we have been able to prove that certain forms of the construction (and some similar phenomena) are provably tractable within the framework of CFPSG's despite previous assertions to the contrary.

The main drawback of the treatment offered above is the proliferation of features. It is not very appealing linguistically to have to add a new feature value each time we add a word to the grammar. One improvement that could be made would treat the reduced form of the construction as involving some sort of lexical reduplication (cf note 4). That's to say, there will be a rule in the lexicon stating that, for every verb X, there is a syntactically identical verb X-bu-X, which has a different semantics. This move will not take us out of the realms of context-freeness, as it can be viewed as a purely local constraint, and has the following advantages. Firstly, it copes more easily with the occurrence of 'ionized' morphemes, and is presumably more adaptable to statements of lexical idiosyncracies in non-verbal classes, than would be a structurally based approach. Secondly, it might form the basis of an explanation for the ungrammaticality of conjoined non-identical verbs (cf examples (22) and (23)).

FOOTNOTES

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1 Taxonomic and transformational analyses are given by Simon (1958) and Wang (1964, 1965) respectively. Wang (1964, 1965) opts unsurprisingly for a conjunction reduction analysis, as does Rand (1969). Li and Thompson (1981, 18.3.2) offer examples, with emphasis on the pragmatics of their use. Huang (1982a, 1982b) gives analyses of A-not-A and related constructions within the theory of GB.

2 In classical Chinese, the morphemes, *xi* and *huan*, were both free, and very close in meaning. As the northern dialects lost syllable-final stop consonants, and with the concomitant threat of a very large number of homophonic clashes, the fusing of near synonyms to form a bisyllabic word was a very common diachronic process. There are other cases in which one might wish to argue that the combination is based on a `specific + general' verbal structure; thus *niangzao*, meaning `brew', consists of the syllable *niang* which was a free morpheme in classical Chinese, with a meaning very similar to the modern compound, and the syllable *zao*, a verb with the very general meaning of `make'. *Niang*, unlike either syllable of *xihuan* is still `semi-free', occurring in the fused verb-object compound

i) niang jiu
brew alcoholic-drink
`brew (beer)' or `make wine'

Most modern bisyllabic words have histories similar to those of *xihuan* and *niangzao*, Li and Thompson (1981:14).

3 Often termed `coverbs' in Chinese linguistics. Prepositions or coverbs have their historical origin in verbs and the occurrences of *gei* in (2) above and (10) below represent the same etymological item. Li and Thompson (1973) claim that coverbs are `syntactically and semantically [...] simply prepositions'. In this essay the terms `preposition' and `coverb' are used interchangeably.

4 I shall assume that the phenomenon of lexical reduplication is not directly implicated here. There are good reasons for so doing. Firstly lexical reduplication does not create strings greater than four syllables long. (Cf Chao (1968:198ff) and Lu (1980:637ff) for general discussion.) Secondly the processes involved in lexical reduplication are clearly not of a unitary nature, as they differ according to part of speech, and are highly idiosyncratic in their applicability.

5 Mai in this sentence does mean 'sell'. It is only tonally distinct from the verb meaning 'buy' that we have already come across.

6 There is one linguistically warranted assumption that must be made, namely that there is a finite number of subcategorization frames. That done, the arithmetic is simple. Chinese has a finite number of syllables, the figure being roughly 2200; verbs in Chinese are either mono- or bisyllabic and the verbal complex may only consist of a verb and a finite number of 'directional complements', the latter drawn from a closed class. The only other source of variation is the possibility of verbs having multiple subcategorization frames, and this variation under the above assumption is limited. As all of the terms in the calculation are finite and non-zero, the total figure for distinct Chinese verbs must itself be finite. The figure is of course large, of the order of one per few hundred Mandarin speakers, but that does not compromise the argument.

7 Alternatively, we might view the subscript i as representing the value of a feature, say [LEXEME i]; each distinct verb is associated with a unique integer and a verb may only be introduced under the category symbol V[LEXEME n] if that verb is associated with the integer n. This would permit the use of a single rule for verb conjunction, but of course leaves us with a feature that has a very large number of values.

8 The argument above also goes through for other constructions that appear to require identity between syllables, such as the lian and the 'concessive' constructions, should we wish to use it (cf DeFrancis (1966:256) and Chao (1968:693). My attention was drawn to these constructions by Steve Harlow). The same is true for the forms of the A-not-A construction that are found with 'potential resultative constructions', as described in eg Tewksbury and Fenn (1967:164, example A1) and Cartier (1972).

9 The difference is clearest in the case of the coverbs ba and bei:

i) Lisi ba Zhangsan da le
Lisi CV Zhangsan hit ASP
'Lisi hit Zhangsan'

ii) Lisi bei Zhangsan da le
Lisi CV Zhangsan hit ASP
'Lisi was hit by Zhangsan'

While these two sentences share the same constituents, a typical GPSG analysis would assign different translation rules to them. In both cases, the object is displaced from its canonical position after the verb, and so some mechanism is necessary to ensure that the arguments to the verb are correctly distributed. In the case of ii), a treatment like that offered by Gazdar (1982:161f) for dealing with English passives is implicated; using lambda abstraction over an NP-type variable inserted into the VP, we may obtain a function which takes the translation of the grammatical subject NP and returns a truth value, with the NP in question properly construed as the object of the verb.

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A NOTE ON
SUBCATEGORIZATION, CONSTITUENT ORDER,
AND THE NOTION `HEAD'*

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In this note, we offer a brief comment on a recent discussion by Richard Hudson of our 1981 paper `Subcategorization, constituent order, and the notion "head"'. Hudson says that the paper "makes three rather separate points" (1983: 446-447; all subsequent quotations from these pages unless otherwise specified), and he attempts to summarize and discuss these three points. Unfortunately, he completely misrepresents all of them. This makes his discussion a strikingly unreliable source of information about what we proposed in our paper. Because the latter provides the basis for a significant amount of published work employing the `ID/LP format' for grammars, it seems sensible to us to try and unravel Hudson's various misunderstandings.

1. Subcategorization

The first point Hudson sees in our paper is that we propose to

> give each construction relevant to subcategorization a number, so that instead of saying that a verb is transitive by saying it has the subcategorization feature [__NP], you call this construction (say) number 37, and then show that a verb is compatible with it by using [37] as a syntactic feature on the verb node.

He says it seems "extraordinary" to him "that this trick should have the appeal that it does have" for us; his reasoning runs as follows:

> If the numbers are just arbitrary diacritics, why not replace them by subcategorization features like [__NP], which are just copies of the constructions concerned - which takes you back to the standard view?

It would indeed be extraordinary if we imagined that there was an interesting difference between representing the features of intransitivity, transitivity, and ditransitivity as [37], [38], [39], rather than [__#], [__NP], [__NP PP] or [Intransitive], [Transitive], [Ditransitive]. The difference between what we propose and what "the

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standard view" (that of Chomsky 1965, henceforth 'Aspects') assumes lies in the interpretation of the formalism in question. Features like [__NP PP], under the Aspects interpretation, are not atomic; they have internal structure. This internal structure is not merely a typographical nicety, a mnemonic for the working linguist: the internal structure plays an essential role in the Aspects theory of lexical insertion. Under this theory, a transformation examines the environment described by the internal structure of the feature and attempts to match it to a restricted class of environments in a tree.

By contrast, in the theory of subcategorization set out in our paper, a feature like [39] has no internal structure (e g it does not decompose into 3 and 9). It is not inspected by any lexical insertion transformation - indeed no such transformation exists. Nor does it, eo ipso, describe an environment in a tree. All it does is index a particular phrase structure rule. As Frank Heny has pointed out, in a passage that we quote in our paper, and as Hudson appears to accept, the Aspects theory is committed to massive redundancy, since "the lexicon will necessarily repeat information time and time again, which is at least in part already extractable from the PS rules" (Heny 1979: 339-340). This redundancy is completely avoided under our account since lexical entries simply contain pointers (to use a useful term from computer science) to phrase structure rules. The redundancy is not eliminated by any 'notational device'; rather it is eliminated by a radically different theory of how subcategorization works.

To see that Hudson is quite wrong in seeing this as a matter of notation, observe that there is a possibility inherent in our theory which does not exist in the Aspects theory. Under our theory, it is possible for there to be more than one rule in a grammar that corresponds to a single Aspects-style subcategorization frame. For example, we might want to have one rule 'VP --> H, VP' for verbs like 'try' and another, syntactically identical (until we consider the rule number), for verbs like 'tend', the two rules having different semantic translations.[1] Our theory can postulate two rules distinct only in their rule numbers in such a case; but the Aspects theory would represent the subcategorization property of a verb that took a VP complement by assigning it the feature [__VP]. There would be no way to differentiate 'try' from 'tend' without the introduction of new diacritic features (parallel to our rule number features, in fact) or other devices entirely outside the subcategorization mechanism. The same would be true for other such pairs. Note for example that 'see' and 'seem' both take a postverbal NP, but they do not have the same syntactic behaviour; 'see' passivizes (cf 'Jim sees everything'; 'Everything is seen by Jim') but 'seem' does not (cf 'Jim seems a nice guy'; *'A nice guy is seemed by Jim').

Although Hudson implies that our proposed change to the theory of subcategorization ("this trick") is merely notational - incorrectly, as we have just seen - he also claims that the Aspects notation has "descriptive advantages". Plainly, this is inconsistent. There can hardly be advantages inherent in the Aspects features but not in ours, if we have only departed from them notationally, as a "trick"; Hudson is adopting Chomsky's familiar rhetorical ploy of saying that on the one hand the proposal is a mere notational variant, and on the other hand that it is wrong. However, inconsistency aside, Hudson's argument for the superiority of the Aspects theory is also mistaken. Hudson argues thus: (i) The verb `joke' can be intransitive or can take one or two PPs as complements; (ii) we would represent this with a lexical entry such as <joke: 1, 3, 8>; (iii) the `standard theory' would represent the subcategorization possibilities of `joke' so as to express "the linguistically simple fact that `joke' takes up to two PP's" (p 447).

Points (i) and (ii) are correct, but (iii) cannot be accepted. It is notable that Hudson omits any `standard theory' representation of the entry for `joke' to illustrate his point. Such an entry will look like either <joke: [__ #], [__PP #], [__PP PP #]>, or <joke: [__(PP)(PP)#]>, depending on the conventions permitted (Chomsky himself handwaved on exactly this point: 1965: 111). To be sure, either one is more mnemonic than <joke: 1, 3, 8>; but that is a fact about the memory abilities of human beings (and human beings who are used to the formalism of Chomsky 1965, at that). Nothing linguistic follows from this mnemonicity in any way. For some reason, Hudson thinks that because the phrase `takes up to two PPs' occurs to him when he looks at the three Aspects features, this has something to do with the generalizations expressible in terms of the Aspects theory; but this is not so. Neither theory makes the predicate `takes up to two PPs' directly expressible - and we think this is as it should be, for the generalization does not deserve to be taken seriously: the real requirements of the verb `joke' are much more specific than that, and involve the type of PP (cf `joke with someone about something', *`joke of someone for something'). Hudson reads too much into the expository exemplification in our sketch.

On the other hand, both the Aspects theory and our own are capable of expressing Hudson's "linguistically simple fact"; and if one insists on trying to decide which expresses it better, the only thing we can think of that might be used to independently differentiate them, using principles that have been used elsewhere in linguistics, is the evaluation metric of orthodox generative phonology, which has simplicity measured by counting symbols. We do not subscribe to such a measure, but those who do may care to note that the notation using the fewest independent symbols is ours. Thus Hudson has offered an entirely spurious argument for preferring the Aspects theory of subcategorization.

2. Constituent order

Hudson's comment on this topic is brief enough to quote in full.

> [Their] second point is that phrase-structure rules should not impose an order on the elements that they introduce, but should leave this job to a different set of rules, called `linearization rules'. This idea has often been aired, and persuasive arguments have often been offered in favour of it, but it doesn't seem to have had a great deal of effect on most users of phrase-structure rules. I wish Gazdar and Pullum success in their campaign.

This comment leaves us wondering whether Hudson actually read our paper, or merely saw what topic we were about to address and guessed at the content. Consider this contrast:

> Hudson's paraphrase of Gazdar and Pullum:
>
> "phrase structure rules should not impose an order on the elements that they introduce"
>
> Gazdar & Pullum (p 108n):
>
> "syntactic representations always display both [dominance and precedence] simultaneously, and so do phrase structure rules"

Phrase structure rules originate in the theory of Post rewriting systems, in which they are defined in terms of strings of symbols. Our observation that they display precedence relations is thus merely analytic.

Furthermore, our paper makes no reference to anything called `linearization rules', though Hudson actually puts this in quotes, as if it came from us. Various workers in differing versions of generative grammar have indeed proposed to have rules defining unlinearized structures which are mapped at a late stage of derivation into ordered structural descriptions of a more familiar sort. But the theory of constituent order developed in our paper has no such mapping, nor any level of unordered structural description, and thus we have no `linearization rules' that "impose an order on the elements" in a constituent.

What we do in fact propose is a way of defining a grammar, i e, a set of phrase structure rules of the ordinary sort, in a way that allows generalizations about linear precedence to be captured as general clauses in the definition. Grammars defined in this way are said to be given in `ID/LP format'. But altering the definition of phrase structure rules is exactly what we do not do, and we have a long footnote (p 108) contrasting our proposal with numerous others on just this point.

3. The notion `head'

The remaining comment concerns the notion `head'. Here again we will quote Hudson in full:

> The third point is that it would be better to treat the notion `head' as a basic category, expressed directly in the grammar, rather than as a derivative notion. Here too I hope their ideas will win an audience, though I should point out that this is a fundamental tenet of dependency theory, as Gazdar and Pullum know, but that they present it as though it was only recently introduced into theoretical linguistics.

Again we note that Hudson's account of our claims contains an exact contradiction of what we actually say:

> Hudson's paraphrase of Gazdar & Pullum:
>
> "The third point is that it would be better to treat the notion `head' as a basic category"
>
> Gazdar and Pullum (p 113):
>
> "Note that the symbol H is not itself the name of a syntactic category ..."

Hudson is right in thinking that we know about the dependency grammar use of a concept to which the term `head' (among various other terms) is sometimes attached. But he appears not to appreciate that the term `head' (like, for example, the term `marked') is one that linguists of many different persuasions employ, often in widely different senses. Zwicky (1985) distinguishes five distinct definitions of `head' employed in morphology and syntax, and distinguishes the dependency grammar notion from all of them. X-bar syntax employs a notion of `head' that sometimes coincides with the referent of the dependency grammar use of the term (e g in the case of VSO sentences) and sometimes does not (e g in the case of coordinate constructions). But such distinctions appear to be lost on Hudson. Once the word `head' appears, it is a knee-jerk reaction for him to think `dependency grammar'. But we do not employ an analog of the dependency-grammar notion of head at all.

Formally, there are major differences between our relation `is the head of' and the one used in dependency grammar. In our X-bar phrase structure theory, the relation `is the head of' holds between daughter and mother nodes in a tree (sometimes the same term is also used to denote the ancestral of the head-of relation). In dependency grammar, relations of dependency hold only between words, and the notion of constituency crucial to every claim of our theory is not available. Our claims in the paper are not only independent of dependency theory, but incommensurable with it in a fairly profound sense.

The claims we make about the notion `head', like the other claims in our paper, are entirely concerned with the statement of a phrase structure grammar. We do not employ any notion of `dependency', and we do not posit `head' as a category. What we do is to introduce a symbol H, interpreted as a copying variable in the sense of Hellan (1977: 90), into certain statements. These statements in part determine a phrase structure grammar via certain conventions, notably the head feature convention, which determines a category corresponding to the H in the rule by reference to feature specifications on the mother (left hand side) category, whose `head' (in one sense) the H in effect represents. As before, we are left wondering whether Hudson actually read any of what we said about this idea and its applications.

4. Conclusion

Hudson begins his discussion of our paper by saying: "this paper makes three rather separate points, and I don't quite see why they were put together into a single article". Let us briefly explain. As Heny (1979) and Hendrick (1980) have pointed out, a major problem with the `standard theory' view of subcategorization concerns lost generalizations about the linear order of sister constituents. Our ID/LP format for grammar definition provides an immediate solution to the problem of expressing the relevant generalizations about the order of the complements that a lexical item subcategorizes for, given the approach to subcategorization that we first outline in our paper. A remaining problem concerns the expression of the generalizations that can be made when the lexical head of a phrase precedes (as in English) or follows all of its subcategorized complements. The theory involving the H variable that we characterize in our paper provides a solution to that problem, inter alia.[2]

In our 1981 paper, (i) we replace Aspects-style context-sensitive subcategorization mechanisms by pointers that associate lexical (sub)categories with the rules that introduce them; (ii) we sever dominance from precedence in the rule definition system without changing the nature of the rules or the structures postulated; and (iii) we capture the notion `head' without assuming any primitive category or relation called `head'. Unlike Hudson, we see these matters as closely and interestingly related.

Concluding his discussion of the book in which our paper appeared, Hudson wonders (p 450): "why is it so difficult (as I have shown) to grasp the main points of some of the papers?" We will not speculate here as to the answer to this question, but we hope that this note will assist Hudson, and perhaps others, in grasping the main points of at least one of the papers.

FOOTNOTES

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[1] This approach is taken in the pre-ID/LP work summarized in Gazdar (1982; written in 1980). It is not taken in Gazdar, Klein, Pullum and Sag (1985), as it happens.

[2] There is a technical difficulty with that solution, pointed out by Shieber (1984), which has caused us to depart from its specific proposal; in Gazdar, Klein, Pullum and Sag (1985: 50), we offer a revised solution that has similar coverage but does not mention H in any LP rule (though H continues to appear in ID rules, of course, for reasons relating to feature distribution).

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CONSTITUENT STRUCTURE IN TEXT-COPYING[1]

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Abstract

In this paper a text-copying task is explored as a naturalistic, reasonably direct, and sensitive psycholinguistic method for the determination of syntactic constituent structure. In two experiments, 70 subjects wrote out copies of typed passages. The points where they paused and looked back to the typed originals were recorded and aggregated across all subjects copying a given passage. The passages differed in content but contained sentences with comparable structure. In general, patterns of pausing were highly correlated across sentences which matched in structure, but reliably distinct in relevant control comparisons. Sentences, clauses and many details of their internal constituency emerge clearly. These details are presented and compared with descriptions from the field of theoretical linguistics. Some suggestive evidence in support of two-stage parsing models is noted.

Introduction

It is a commonplace of linguistics that sentences are more than just concatenations of words. They have hierarchical constituent structure: words are grouped into phrases and these, in turn, may form other phrases or clauses, which finally make up sentences. Zwicky (1978) provides a valuable conspectus of the reasons which have led theoretical linguists to accept constituents intermediate between the word (or morpheme) and the sentence. Essentially, the recognition of constituent structure facilitates - and in some cases appears to be an indispensable basis for - generalizations about syntactic, semantic and even some phonological patterns. Psycholinguistic research, some of which enters Zwicky's survey and some of it to be mentioned below, has established that hierarchically-organized constituents also play a part in the mental processes that subserve human language use.

If it could be demonstrated that, in studies of sentence constituency, psycholinguists and theoretical linguists are looking at fundamentally the same phenomena (though from different angles) then the latters' accounts of syntactic structure could be useful to psycholinguists trying to understand such processes as sentence comprehension. If, furthermore, psycholinguists can develop

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good ways of revealing the sentence constituents that language users actually employ in listening, reading, writing and talking, then their results could be a worthwhile addition to the empirical data-base of theoretical linguistics.

An ideal psycholinguistic method for probing the constituency of sentences should be strong on the following three characteristics: task naturalness, simultaneity of subject's language processing and experimenter's observation, and sensitivity to details of constituent structure. The first two requirements relate to validity: findings from highly artificial tasks cannot confidently be generalized to normal language use, and time elapsing between processing and observation allows scope for other variables to influence the results and thereby complicates their interpretation (the second desideratum is but a slight extension of Levelt's (1978:4) notion of "simultaneous measurement"). Sensitivity to detail is needed partly to enhance reliability, but also to enable one to gather facts beyond those which are anyhow obvious.

Levelt (1969, 1970) has found ways of eliciting judgements of the relative strength of the relationships between all possible pairs of words in a sentence. Hierarchical cluster analysis techniques translate matrices of these judgements into tree diagrams. The method appears to be sensitive, but the judgemental task is not one that figures in everyday language use and the subjects' judgements do not coincide in time with their initial processing of the sentences. Excepting perhaps for naturalness, the same points can be made about Johnson's (1965) analysis of transitional error probabilities: subjects memorized sentences and their recall protocols were examined to determine, across a group of subjects, the probability of error in each transition between the successive words of a given sentence. Martin (1970:160) simply asked subjects to "... arrange the words of each sentence into natural groups ... into whichever and however many parts seem intuitively natural". The nature of the resulting cluster analysis trees speaks for the sensitivity of the method, but again the judgements followed whatever processing the subjects did on first exposure to the sentences and the judgements were, as in Levelt's (1969, 1970) work, metalinguistic ones that language users need never have ordinary occasion to make overtly. The approach is also so transparent as to amount to nothing more than a systematic collation of opinions.

The adaptation of subjective click location tasks by Bever and his associates for the psycholinguistic investigation of syntactic constituency is well known (see Fodor, Bever & Garrett, 1974, for a synopsis). Subjects hear a sentence with a superimposed click and afterwards (i.e. the method is not a simultaneous observation one) indicate where in the sentence they thought they heard the click. The clicks are held to drift towards - and/or to be more accurately located when they occur in - certain types of inter-constituent boundary. The method has been cogently criticized by Larsen (1971), and Olson & Clark (1976) also raise important questions about the interpretation of click experiment results. Here it will suffice to add that the task is not a natural one and it appears, from Bever, Lackner & Kirk's (1969) study, to be somewhat insensitive to constituent structure below clause-level.[2]

The inferring of constituency from hesitation pauses in spontaneous speech (see Hawkins, 1971, for an example of this genre) and from pausing when subjects read aloud (as exemplified by the research of Grosjean and his collaborators, e.g. Grosjean, Grosjean & Lane, 1979) strike me as more promising directions. Speaking and reading are natural tasks (the latter, of course, only in literate communities) and simultaneity is assured by the fact that pausing is part of the performance. The work of Grosjean et al suggests that their method is sensitive to details of sentence structure. Hesitation pause analyses, such as Hawkins', seem less sensitive to clause-internal structure; perhaps because of the common practice of ignoring very short pauses (e.g. below 250 or 300msec, on the grounds that some of these may be merely stop consonant closures). For present purposes, the data from hesitations in spontaneous speech have two further drawbacks: some of the pausing may be occasioned by the speaker hunting for content words (rather than by the category of constituent being readied for output) and, secondly, the investigator cannot choose exactly which sentences the speakers will utter. Reading aloud eliminates these two problems, since the investigator specifies what the subjects are to read. There is a residual possibility, however, that both types of pause study may include some pause time which is not germane to the concerns of this paper: namely, it is conceivable that pause duration in speech could have other signalling functions besides the demarcation of constituents (see Butterworth, 1980:157).

The methods enumerated above are by no means the only ones that have been employed, but they are a representative selection. None of them yields constituent structure analyses which exactly match the clear cases over which there is a reasonable concensus within theoretical linguistics. Of course, the theoretical linguists might be mistaken, but it might alternatively be the case that the currently available psycholinguistic methods are too much subject to random errors and confounding factors. The method of text-copying presented in this paper involves observation that is nearly concurrent with the processing, shows considerable sensitivity to detail and, by and large, gives hierarchical constituent structures closer to those of theoretical linguistics than are produced by any other technique known to me. It is also (see footnote 2) an economical method. For literate subjects the task is a reasonably naturalistic one. It amounts to doing something which is occasionally done anyway during the normal course of one's life as a language user. The processing constituents derived from data collected in this way are thus likely to have ecological validity and are unlikely to represent merely ad hoc strategies resorted to for coping in a novel experimental situation.

The first of the two experiments described below (immediately following an account of the rationale for the text-copying method) was mainly a methodological exploration. The second experiment was an attempt to use the method to examine particular structures which are of some interest to theoretical linguists.

Rationale[3]

Subjects were asked to write out copies of typed passages (just as one might copy out a quote from an academic article). The typed originals and the sheets of paper on which subjects made their copies were placed sufficiently far apart to prevent subjects from reading the original and watching their pens at the same time. In these circumstances, subjects who choose to guide their writing visually (and, fortunately, none of those in the two experiments reported below chose to write completely blind of visual control) are obliged to look at the typed original and take a stretch of it into short-term memory, then to write down that stretch from memory before looking back at the original for another stretch to load into short-term memory, etc. In principle, these stretches of material could be of, at least, the following two kinds:

> They could be linguistic constituents of whatever size the subject found convenient.
>
> Or they could be arbitrary strings of words, e.g. one word at a time or five words at a time.

The motivating hypothesis for the two studies presented here is that subjects will generally follow the first strategy.

If different subjects copying the same sentence transfer constituents of different sizes, their aggregated results will reveal the hierarchical structure of the sentence. To see why this should be so, consider a two-clause sentence in which each clause has two short phrases as its intermediate constituents:

$$_{S}[_{Cl}[\text{PHRASE PHRASE}]_{Cl}[\text{PHRASE PHRASE}]].$$

Imagine that it is copied by three subjects who differ in short-term memory capacity (or preference or perhaps even just in rashness): Subject A copies a whole sentence at a time, Subject B copies a clause at a time and Subject C copies only a phrase at a time. For these three subjects the aggregated record of look-back-for-another-chunk-to-copy pauses will be (with the subscript letters showing where each subject paused):

$\text{PHRASE}_{\text{C}}\text{PHRASE}_{\text{BC}}\text{PHRASE}_{\text{C}}\text{PHRASE}_{\text{ABC}}$

It is clear that the number of pauses in each locus will rather directly reflect the constituent structure of the sentence; and there is no reason why, with more and varied copiers, the internal structure of phrases should not be revealed too.

There is a straightforward algorithm for converting numbers of pauses made by a group of people copying a sentence into a bracketing which represents the hierarchical constituent structure which that group imposed on that sentence. It is illustrated by means of some data collected in Experiment 2. Forty-seven subjects copying a set of five-word sentences yielded the following pattern of pauses:

$\text{ARTICLE}_{2}\text{NOUN}_{9}\text{MODAL}_{3}\text{BE}_{8}\text{PAST-PARTICIPLE}_{40}$

(that is, 2 of the subjects paused after the article, 9 after the noun, ... and 40 of the 47 stopped writing and looked back to the original text when they came to the end of the sentence). The procedure[4] is: (a) Find the smallest cluster of pauses (in this case it is that of the two people who paused after the article) and bracket together into a single constituent the constituents which immediately flank that cluster. (b) Delete that cluster of pauses and return to step a. For the specimen data, successive passes through steps a and b give:

$[\text{ARTICLE NOUN}]_{9}\ \text{MODAL}_{3}\ \text{BE}_{8}\ \text{PAST-PARTICIPLE}_{40}$

$[\text{ARTICLE NOUN}]_{9}\ [\text{MODAL BE}]_{8}\ \text{PAST-PARTICIPLE}_{40}$

$[\text{ARTICLE NOUN}]_{9}[[\text{MODAL BE}]\ \text{PAST-PARTICIPLE}]_{40}$

$[[\text{ARTICLE NOUN}]\ [[\text{MODAL BE}]\ \text{PAST-PARTICIPLE}]]_{40}$

This bracketing is of course equivalent to the tree diagram in Figure 1, where the clusters of pauses have been resuscitated to show the relationship between them and the resulting tree.[5] To a linguist this tree is an attractive invitation to deploy the labels S, NP, VP and AUX.[6] The algorithm, however, does not provide the labels; that is a matter for subsequent interpretation.

As it has been stated thus far, the algorithm makes no allowance for ties. A rider added to the end of step a will rectify this: "If two or more clusters of pauses tie for the position of smallest cluster, deal with them simultaneously". Thus $T_5U_2V_7W_2X_2Y_2Z_{10}$ will be mapped by a single pass through the procedure into:

$$T_5\ [U\ V]_7\ [W\ X\ Y\ Z]_{10}.$$

The only point that remains to be made about the rationale for the text-copying method is that effects attributable to *sentence structure* can be disconfounded from the pause-inducing propensities, if any, of *particular words* by amalgamating results from two or more sentences which are structurally comparable but contain different words.

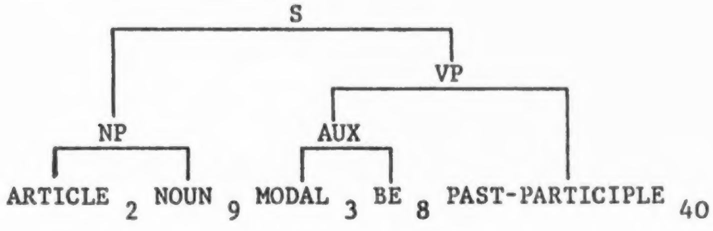

Figure1. Tree diagram based on the amalgamated pause distribution of a set of four 5-word sentences used in Experiment 2.

Experiment 1

Subjects

Forty-six senior undergraduate and beginning graduate Linguistics students participated in the experiment in fulfilment of a course requirement. Psycholinguistic investigations of constituent structure had not yet been discussed in the course at the point when the experiment was conducted. Half of the group, 23, acted as subjects and the remaining 23 were observers.

Materials

Four typed 62-word passages (see Figure 2, opposite) were used. The COHERENT 'SEMANTICS' and COHERENT 'PHONETICS' passages were loosely derived from introductory textbooks (and were therefore expected to strike the Linguistics student subjects as reasonably familiar and not particularly abstruse). The two JUMBLED passages are variants of the corresponding COHERENT passages with the words randomly re-ordered. Lineation and the minimal punctuation were exactly as shown in Figure 2.

Each of the COHERENT passages contains four sentences. They were designed to correspond in their syntactic structure across passages. The first sentence in the COHERENT 'SEMANTICS' passage (henceforth, sentence S_1) is 14 words long and has a clause 'extraposed' from subject position. This was matched by the fourth sentence in the COHERENT 'PHONETICS' passage, P_4, which is also 14 words in length and also has a clause 'extraposed' from subject position. The second COHERENT 'SEMANTICS' sentence, S_2, is

COHERENT 'SEMANTICS'

It is common knowledge that children will often misunderstand the meaning of a word. In the vast majority of cases such misconceptions will be corrected by later experience. If this does not happen a semantic change will take place in the usage of the new generation. A further factor which may lead to changes in meaning is the loss of formal motivation.

COHERENT 'PHONETICS'

A notable problem which has made vowels difficult to transcribe is the absence of articulatory contact. When the consonants are formed a trangible stricture is usually present between the surfaces of the two articulators. More than half of the time this contact can be sensed by the investigator. It is generally recognized that phoneticians may sometimes misclassify the quality of a vowel.

JUMBLED 'SEMANTICS'

If experience this later does by not corrected happen be lead that to a will semantic misconceptions change such will cases knowledge changes take of place majority in vast the the usage in of common in is meaning word the a new of generation meaning a the further it is motivation of misunderstand factor often which will may children formal loss the.

JUMBLED 'PHONETICS'

The between surfaces present of usually the is two stricture phoneticians articulators tangible more a than formed half are of consonants problem the true time when this contact articulatory can misclassify a vowel of be absence sensed the by is the transcribe investigator to quality it difficult is vowels generally made recognized has that which the phoneticians problem may notable sometimes a.

Figure 2. The four passages used in Experiment 1. Excepting for the headings, which did not appear on the versions actually used, the format of each passage is shown exactly as it was presented to the subjects. Two errors that appeared in the originals are retained, uncorrected, here: *tangible* was incorrectly typed as *trangible* in the COHERENT 'PHONETICS' passage and the word *phoneticians* appears once too often in the JUMBLED 'PHONETICS' passage.

likewise equal in length (14 words) and comparable in structure (passive with a preposed adverbial phrase) to the penultimate COHERENT 'PHONETICS' sentence, P_3. S_3 and P_2 are both 18 words long and have two-clause subordinating conjunction structures. S_4 and P_1 match in having 16 words each and in having relative clauses on their subject NPs.

For purposes of analysis, the JUMBLED passages are treated as if they were each composed of four _sentence counterparts_ equal in length to the corresponding parts of the relevant COHERENT passage. Thus (using prime marks to denote these random strings of words) the 16-word sentence at the start of the COHERENT 'PHONETICS' passage, P_1, has as its control counterpart P'_1, the first 16 words of the JUMBLED 'PHONETICS' passage ("The between ... than"); and so forth.

Procedure

Testing took place in two separate sessions (with any given subject attending only one of the sessions) in a language laboratory. Each subject made written copies of two of the typed passages. An arbitrarily selected half of the subjects at each testing session copied the COHERENT 'SEMANTICS' and JUMBLED 'PHONETICS' passages. The remaining subjects copied the COHERENT 'PHONETICS' and JUMBLED 'SEMANTICS' passages. Half of each of these sub-groups copied their COHERENT passage first and half did their JUMBLED passage first.

The experimental lay-out for right-handed subjects was as follows. In front of the subject was a blank sheet of paper on which the copy was to be written and a few inches to the right of the subject's right elbow was the typed passage to be copied. This arrangement necessitated a distinct swivel of the subjects' heads when they looked up from their writing to scan the original from which they were copying. To the left of the subject sat the observer. Observers had before them identical typed copies of the particular passages which their subjects were copying. On these, observers simply made a mark after the last thing their subject had written each time the subject stopped writing and glanced back at the original passage on the right. For left-handed subjects the lay-out was reversed: the observer sat on the subject's right and the original passage was placed to the left of the subject's left elbow.

Subjects and observers heard separate tape recorded instructions through language laboratory headsets immediately before the start of testing. Subjects were asked to make "quick and accurate" written copies of the typed passages. The instruction was reiterated as "aim for accuracy but work reasonably quickly too". They were told that their individual observers would be watching them writing and would be "noting something about the way you write". They were asked not to move the typed passages away from the position beyond their writing elbows.

The task of noting the points in the passages at which subjects stopped writing and looked back at the original across their writing arms was explained to observers. They were told to keep the original passages covered until they and their subjects were ready to start. They were instructed to allow a rest of approximately one minute between the copying of the first and the second passage, and to substitute the second passage - covered until copying was about to start - for the first during this interval. For observers the purpose of the experiment was characterised as an attempt to "find out where people stop when they are making a written copy of a text". They were offered no further information regarding the hypothesis under investigation. Both subjects and observers were promised that a fuller explanation would be given at the end of the experiment (and this was duly delivered).

Results

Half of the subjects had copied a COHERENT passage before copying a JUMBLED one and half of them had copied JUMBLED before COHERENT. Inspection of their separate results revealed only one striking difference: the 5 subjects who copied JUMBLED 'SEMANTICS' as their first passage paused on average 27.20 times in dealing with this particular passage, whereas the 7 who had it as their second passage yielded a mean for this passage of 19.43 pauses. There is no ready explanation for this difference and for the other three passages order differences appear to be negligible. Order effects were therefore ignored and data

n		COHERENT 'SEMANTICS'		JUMBLED 'PHONETICS'
11	mean	11.18	mean	18.73
	sd	5.71	sd	8.83

n		COHERENT 'PHONETICS'		JUMBLED 'SEMANTICS'
12	mean	17.17	mean	22.67
	sd	6.70	sd	6.91

Table 1. Means and standard deviations of pauses per subject for the four passages of Experiment 1. Passage-final pauses, where everyone had of necessity to stop writing, are not counted here and are omitted from all subsequent analyses.

from subjects copying a given passage either first or second were combined to produce the means and standard deviations given in Table 1.

Subjects looked back to consult the original text more often when copying JUMBLED passages than when copying COHERENT passages (one-tailed paired-observation t-tests give t=3.647 (df=10, p<.005) for COHERENT 'SEMANTICS' versus JUMBLED 'PHONETICS' and t=2.312 (df=11, p<.025) for COHERENT 'PHONETICS' versus JUMBLED 'SEMANTICS').

These differences arise from subjects carrying across sequences of just over four words long (mean = 4.05) from eye to pen before each pause for COHERENT texts, but fewer than three words (mean = 2.85) each time for the JUMBLED texts. Only 5 of the 23 subjects defied this trend and paused more often in their COHERENT passages; in each of these cases the difference was small and 4 of these five reversals occurred when JUMBLED 'SEMANTICS' followed COHERENT 'PHONETICS'.

Although the two JUMBLED passages do not differ significantly in the average numbers of pauses which they attracted (two-tailed independent-sample t=1.198, df=21, .30>p>.20), the COHERENT 'PHONETICS' passage was clearly more difficult than the COHERENT 'SEMANTICS' passage (two-tailed independent-sample t=2.296, df=21, p<.05). A factor which might contribute to this is that the 'PHONETICS' passages contained more long words. Note in Figure 2 that the 'PHONETICS' passages extend over six lines, versus five lines for the 'SEMANTICS' passages, despite their having the same number of words as the 'SEMANTICS' passages. This is highlighted by pauses that fell within words: there were only 4 intra-word pauses, in total, for the 'SEMANTICS' passages (all of them in the JUMBLED version), but the 'PHONETICS' passages accumulated a total of 23 such 'spelling check' pauses (8 for the COHERENT version and 15 for the JUMBLED version). In compiling Table 1 and for the purposes of all subsequent analyses, a pause within a word was treated as if it had occured after that word (and ignored altogether if that subject had also paused after the word). Compared to the total of more than 800 pauses, the number of intra-word pauses is in any case trivially small.

The JUMBLED versions of the passages were intended to provide between-subjects control comparisons for any syntactic effects on pausing in the corresponding COHERENT passages. The mean of 11.18 pauses for the COHERENT 'SEMANTICS' passage is indeed highly significantly different from the figure of 22.67 for JUMBLED 'SEMANTICS' (one-tailed independent-sample t=4.324, df=21, p<.005), but the difference between the two 'PHONETICS' passages fails to reach significance (one-tailed independent-sample t=0.480, df=21, .35>p>.30). The differences in difficulty between the 'PHONETICS' and the 'SEMANTICS' passages catalogued in this and the preceding paragraph would have provided an excuse had syntactic effects attributable to parallels between the sentences of the two COHERENT passages failed to manifest themselves. However, the correlations discussed below largely obviate the need for excuses.

The minimal syntactic effect that one might expect would be large concentrations of pauses at the ends of sentences in the COHERENT passages. (The end of the fourth sentence in each passage, of course, corresponds to the end of the whole passage. Since all subjects had to stop when they came to the end of a passage, this expectation can only be tested for the first three sentences in the COHERENT passages.) Table 2 shows that the subjects overwhelmingly paused to look back at the original text when they reached the full stops at the ends of the first three sentences. It could perhaps be argued that a full stop followed by a double-letter space followed by a capital letter is an eye-catching configuration that might induce pausing regardless of its role in our orthographic code; that even subjects who were treating the copying task as somehow only marginally a linguistic task might pause there simply because the signal is so salient. The right-hand column of Table 2 is offered as a counter to this hypothetical objection. Typed lines come to an end for non-linguistic reasons. The width of the paper obliges the typist, and the reader, to stop the rightwards traverse and return to the left margin. Notwithstanding this natural break which line ends present, subjects took them as opportunities for pauses only 27% of the time. They were clearly far more willing to copy on beyond the end of a typed line than to do so at the end of a sentence. Having been dealt with here, pausing at full stops is henceforth ignored, i.e. only sentence-internal pauses are considered in the analysis that follows.

	Percentages of opportunities taken to pause	
	at full stops	at ends of typed lines
COHERENT 'SEMANTICS'	82	27
COHERENT 'PHONETICS'	86	27

Table 2. Pausing at ends of sentences and at ends of typed lines.

For each of the four passages, subjects' pause records were amalgamated to produce pause distributions showing how many subjects paused after each word in that passage. Product-moment correlation coefficients were used to evaluate the extent to which the distribution of pauses across the successive inter-word gaps within a given sentence matched the pause distribution of the syntactically comparable sentence in the other COHERENT passage. As control comparisons, product-moment correlations were also computed between the pause distribution for each sentence

		COHERENT 'SEMANTICS'				JUMBLED 'SEMANTICS'				COHERENT 'PHONETICS'			
		S_1	S_2	S_3	S_4	S'_1	S'_2	S'_3	S'_4	P_1	P_2	P_3	P_4
COHERENT 'SEMANTICS'	S_1												
	S_2	.54											
	S_3												
	S_4												
JUMBLED 'SEMANTICS'	S'_1	.18											
	S'_2		-.12										
	S'_3			.15									
	S'_4				-.28								
COHERENT 'PHONETICS'	P_1				.46								
	P_2			.69**									
	P_3	.32	.56*										
	P_4	.83**	.53									.37	
JUMBLED 'PHONETICS'	P'_1								.11	-.34			
	P'_2							-.47			-.13		
	P'_3						-.08					.28	
	P'_4					.17							.41

Table 3. Matrix of correlations for pause distributions in Experiment 1. ** highly significant, $p<.01$; * significant, $p<.05$. The sentences differ in length. Thus size of coefficient needed for significance varies. Unprimed abbreviations denote real sentences. Primedabbreviations are for the 'sentence counterparts' in the JUMBLED passages. Subscripts indicate the position within a passage of a given sentence or 'sentence counterpart'. Sentence-final pauses are excluded from the analysis.

and the distribution of pauses within its random 'sentence counterpart' in the corresponding JUMBLED passage, and between the pairs of pause distributions of these random 'sentence counterparts' across passages. Furthermore, since each of the COHERENT passages contained two equally long (14-word) sentences, it was possible to compute correlations comparing syntactically different structures, for these particular sentences. Table 3 (previous page) presents all the correlation coefficients.

The four underlined coefficients are the only ones in Table 3 which represent pause distributions for syntactically comparable sentences. Three of these four coefficients are significantly above chance level - two of them highly so. The remaining sixteen coefficients in Table 3 are all ones which were computed as control comparisons and none of them rises above chance level.

Discussion

Table 1 shows that some property, or combination of properties, of the COHERENT passages made them easier to copy in larger chunks than the JUMBLED passages. Table 2 shows that subjects were very sensitive to the division of the COHERENT passages into sentences. And Table 3 provides good grounds for believing that the internal structure of sentences is the major determiner of where subjects stop writing and look back for more when they are copying. The argument is that (with one exception which will receive further discussion below, viz. S_4xP_1) the pause distribution of a sentence correlates strongly only with that sentence in the other passage which was designed to exhibit comparable constituent structure. It fails to correlate significantly with the pause distribution for a random stretch of words located in the corresponding position in the JUMBLED version of the passage (and pairs of these random 'sentence counterparts' do not yield significant intercorrelations). Furthermore, for the 14-word sentences, the correlation coefficients fail to reach significance when sentences with different syntactic structures are compared.

Figure 3 presents tree diagrams derived from the pause distributions of the sentences in the COHERENT passages. Pairs of sentences intended to have comparable structures arealigned and the pauses falling in corresponding gaps are summed to provide the data upon which the trees are based. The tree for a pair of sentences represents their shared structure to the extent that it emerges through experimental 'noise'. The non-terminal node labels have been added on a purely intuitive basis whenever these appear to be justified by the strings they dominate (given their particular sentence contexts). It is my contention that the mere possibility of straightforwardly supplying theoretical linguistic labels for 30 nodes out of 48 (ie 62.5%) is a strong indicator that linguistic constituency influenced pausing. Incidental support for this position is gained by examining parts of the sentences that were not fully syntactically comparable across the two passages. These design blemishes are the underlined strings of words in Figure 3. The relevant observations are: in S$_1$ there is only one pause within the NP common$_1$knowledge, but four

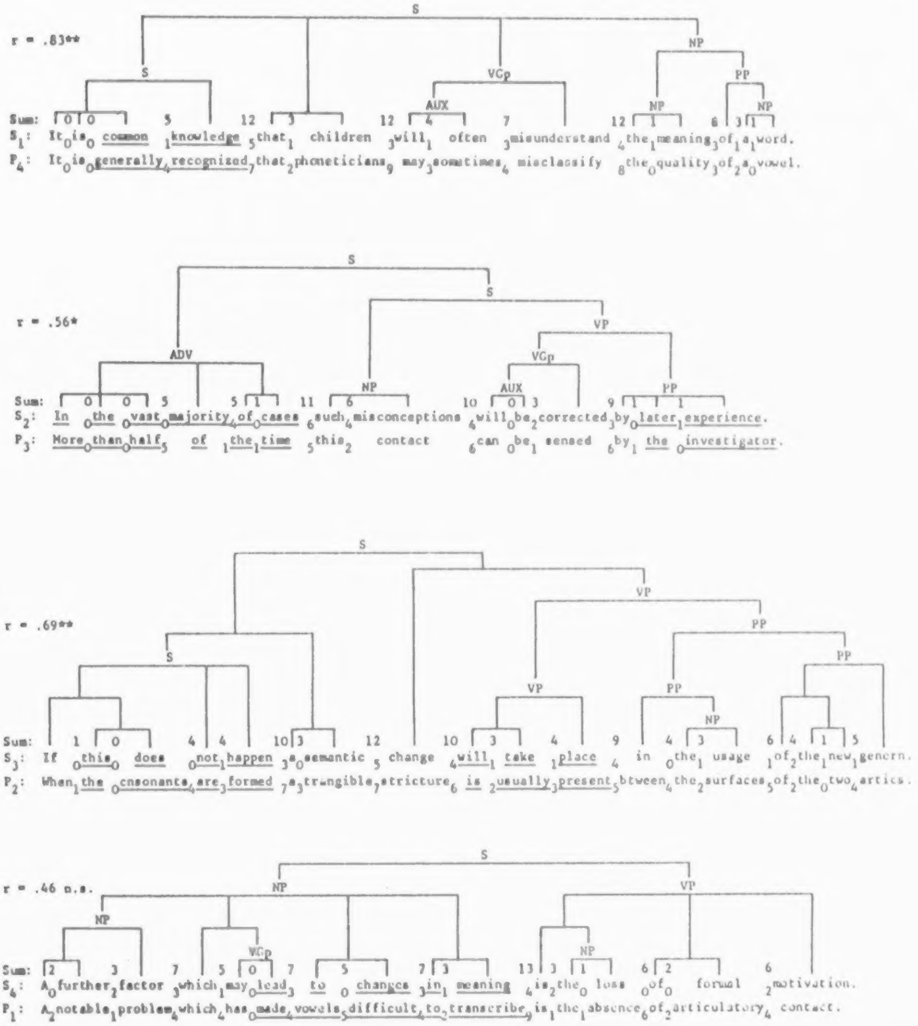
r = .83**
S
S
VCp
AUX
NP
PP
NP
NP
Sum: 0 0 5 12 3 12 4 7 12 1 6 3 1
S1: It 0 is 0 common 1 knowledge 5 that 1 children 3 will 1 often 3 misunderstand 4 the 1 meaning 3 of 1 a 1 word.
P4: It 0 is 0 generally 4 recognized 7 that 2 phoneticians 9 may 3 sometimes 4 misclassify 8 the 0 quality 3 of 2 a 0 vowel.
r = .56*
S
S
VP
ADV
VCp
NP
AUX
PP
Sum: 0 0 5 5 1 11 6 10 0 3 9 1 1
S2: In 0 the 0 vast 0 majority 4 of 0 cases 6 such 4 misconceptions 4 will 0 be 2 corrected 3 by 0 later 1 experience.
P3: More 0 than 0 half 5 of 1 the 1 time 5 this 2 contact 6 can 0 be 1 sensed 6 by 1 the 0 investigator.
r = .69**
S
VP
PP
S
PP
VP
PP
NP
Sum: 1 0 4 4 10 3 12 10 3 4 9 4 3 6 4 1 5
S3: If 0 this 0 does 0 not 1 happen 3 a 0 semantic 5 change 4 will 1 take 1 place 4 in 0 the 1 usage 1 of 2 the 1 new 1 genern.
P2: When 1 the 0 cnsonants 4 are 3 formed 7 a 3 trangible 7 stricture 6 is 2 usually 3 present 5 btween 4 the 2 surfaces 5 of 2 the 0 two 4 artics.
S
r = .46 n.s.
NP
VP
NP
VCp
NP
Sum: 2 3 7 5 0 7 5 7 3 13 3 1 6 2 6
S4: A 0 further 2 factor 3 which 1 may 0 lead 3 to 0 changes 3 in 1 meaning 4 is 2 the 0 loss 0 of 0 formal 2 motivation.
P1: A 2 notable 1 problem 4 which 4 has 0 made 4 vowels 5 difficult 4 to 2 transcribe 9 is 1 the 1 absence 6 of 2 articulatory 4 contact.

Figure 3. Tree diagrams for the amalagamated pause distributions of the sentences in the Experiment 1 COHERENT passages. The relevant correlation coefficients from Table 3 are reproduced beside the trees. Parts of thesentences that are not fully syntactically comparable across the two passages are underlined. A few long words have been contracted in sentences S$_3$ and P$_2$. See Figure 2 for full versions of these two sentences.

pauses separate the adverb and the verb in the falsely matched string generally$_4$recognized of P_4; in S_2 ...$_0$majority$_4$[of$_0$cases] received a distinct bracketing from its false match in P_3, ...$_5$[of$_1$the$_1$time], with the preposition phrases being respected both times; the subject NP of the first clause of P_2, the consonants is separated by four pauses from its VP, are formed, but the linearly corresponding gap in S_3, between does and not, attracted no pauses (a difference that many theoretical linguists might have predicted); and within the relative clauses of S_4 and P_1 the individual bracketings almost approach being theoretically reasonable,

...[may$_0$lead]]$_3$[to$_0$changes]$_3$[in$_1$meaning]
versus
...[has$_0$made]$_4$vowels]$_5$[difficult$_4$[to$_2$transcribe]].

These observations suggest that the text-copying method is considerably more sensitive to syntactic detail than had been anticipated. The mismatches underlined in Figure 3 are design blemishes only when the criteria for syntactic comparability exceed a certain level of delicacy. When the passages were being prepared no great sensitivity to detail was expected and it was assumed that one 7-word relative clause was as good as any other 7-word relative clause, one 6-word adverbial phrase a suitable match for any other 6-word adverbial phrase, etc. It is very likely that the four crucial correlation coefficients in Table 3 would have been enhanced in size if the pairs of sentences had been more closely matched. The S_4xP_1 correlation, which at 0.46 just fails to reach significance ($.10>p>.05$), could well have been raised to significance if the two relative clauses had had the same internal structure. In Experiment 2, reported below, closer structural parallels were striven for.

An important caution is evident from inspection of Figure 3: the text-copying method is clearly rather insensitive to syntactic detail at the beginnings of sentences; at least this appears to be true for highly literate subjects transferring, on average, four words of text at a time. Even though It at the start of sentences S_1 and P_4 is a major constituent, no subjects were content to take in only that one short word and write it down before proceeding to deal with more of the passage. After the first few words of a sentence, however, it appears that the fact that different subjects copied sequences of different lengths led to asynchronies which ensured a fair distribution of pauses among the remaining constituent breaks.

Word-length

Content words tend to be longer than grammatical function words and, in English, phrases generally start with a grammatical function word and end with a content word (eg a noun phrase such as *an armadillo* is opened by a short function word and closed by a long content word). Conceivably the patterns exhibited in Figure 3 are simply a reflection of word-length: subjects take in one or more short (function) words and then, still having short-term memory space to spare, they take in a following long (content) word: this fills the available capacity and the sequence is then copied out before they pause and return to the passage for another chunk of text.[7] If this were so, one might either dismiss the results as artefactual or, alternatively, develop a speculation to the effect that the relationship between word-length and the function-word/content-word distinction was a natural adaptation to the ecological pressures of linguistic processing. In fact, however, the JUMBLED texts provide evidence that the function/content distinction is efficacious over and above its connection with word length. Of course, the differences in numbers of pauses between COHERENT and JUMBLED texts (Table 1) already indicate that something else in addition to mere word-length is implicated.

Figure 4 (opposite) shows the median numbers of pauses after words of different lengths in the two JUMBLED passages. (Medians are used because some of the distributions are skewed. The oddity of providing medians for the two 5-syllable words and the lone 6-syllable word is recognized.) The open circles depict the pattern for all 122 words (the two passage final words are omitted from the analysis). Numbers of pauses clearly do not simply increase in proportion to the number of syllables in a word. Rather, there is an enormous leap between 1-syllable words and 2-syllable words. The relationship could be idealized by saying that monosyllabic words attract small numbers of following pauses (Median = 2.24), but for words of more than one syllable the number of pauses fluctuates on either side of a high plateau. The horizontal line at 5.81, representing the median for 47 of the 48 polysyllabic words is an indication of where this plateau might be. (The preposition *between*, the only polysyllabic function word that happened to appear in these passages was omitted from the calculation. Its inclusion would scarcely alter the figure of 5.81 for the median.)

The 74 monosyllabic words comprised 16 content words (nouns, full verbs, adjectives and adverbs) and 58 function words. Their two separate medians (3.50 and 2.00, respectively) are given as *xs* in Figure 4. A median-test comparison of the two types shows that monosyllabic content words attracted a highly significantly greater number of pauses than monosyllabic function words ($X^2_c=6.82$, $p<.01$, df=1). Thus 1-syllable content words reach up towards the plateau for polysyllabic words. They do not quite reach it, however. By the median test, they attract significantly fewer pauses than do the 47 polysyllabic content words ($X^2_c=5.04$, $p<.05$, df=1).

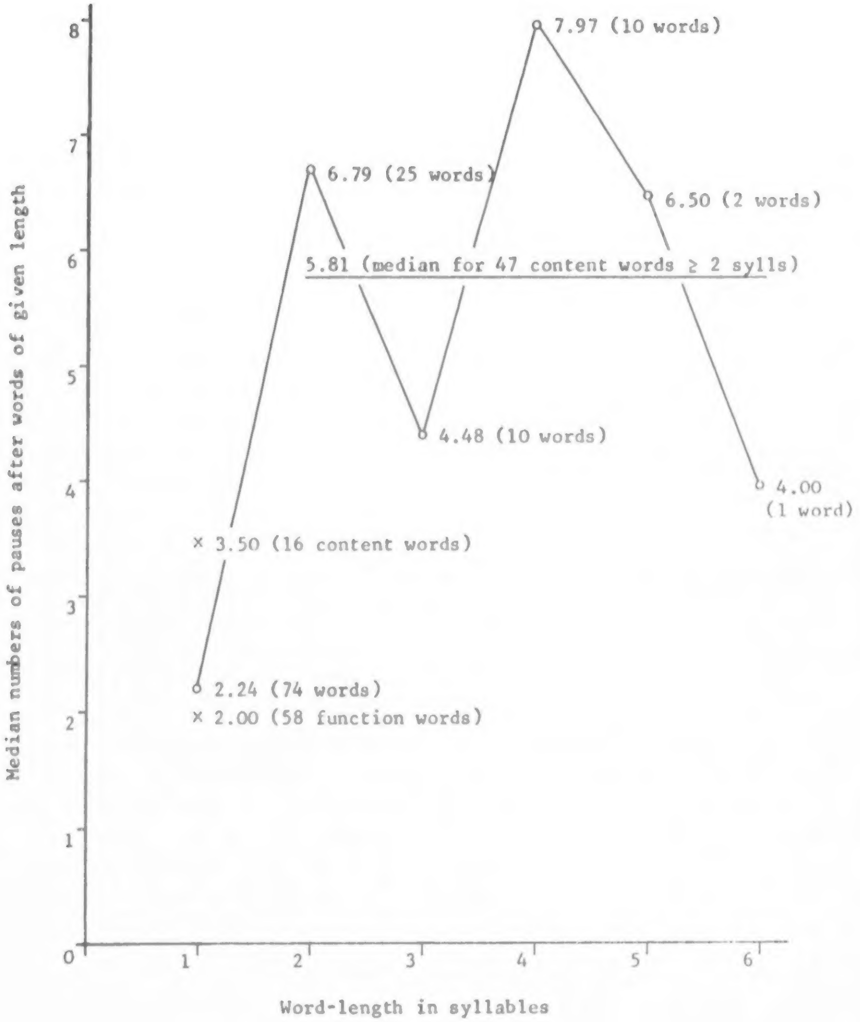

Figure 4. Median numbers of pauses after words of different lengths in the JUMBLED passages of Experiment 1. Passage-final pauses are excluded. Open circles are data points for all words. The xs show content words and function words separately, for monosyllables. For each data point the number of contributing words is indicated in brackets.

Any given random ordering is bound to yield some sequences that look like valid constructions. See, for example, the apparent VP can misclassify a vowel in the third line of the JUMBLED 'PHONETICS' passage (figure 2). To isolate word effects from constituent structure effects it would have been better to have given the JUMBLED passages in a different random order for every subject. Practical constraints dictated that only a single randomized version of each passage be prepared. Nonetheless it appears incontestable that although word-length does influence pausing - manifested in the difference between monosyllabic and polysyllabic content words - the syntactically relevant distinction between content words and function words is also an important (and perhaps greater) factor.

Experiment 2

In view of the text-copying method's sensitivity to the details of constituent structure, suggested by the results of Experiment 1, it was decided to employ it with sentences exhibiting closer structural matches across passages. The second experiment used only COHERENT passages, because they had already been clearly demonstrated to evoke different behaviour from that seen with the JUMBLED passages.

An attempt was also to be made to use the text-copying method to elucidate the constituency of two particular construction types, regarding the analysis of which there is contention among theoretical linguists. Firstly, prepositional verbs (e g allow for). Does the preposition form "a semantic and syntactic unit with the verb ...." similarly to phrasal verbs such as find out, as claimed by Quirk, Greenbaum, Leech & Svartvik (1972:812) or is the preposition in closer construction with a following noun phrase, as it would be in an ordinary verb-followed-by-preposition-phrase-adjunct construction (V-PP) such as: speak[for a period of several seconds]? The question is whether prepositional verbs, such as allow for are appropriately bracketed [allow for] or allow [for NP].[8] Secondly, it seemed worth trying to find evidence for differences between the constituent structures of restrictive and non-restrictive relative clauses.[9]

A final aim of Experiment 2 was to investigate whether structurally ambiguous sentences would yield distinct pause distributions in contexts designed to favour different interpretations of them.

Subjects

Ninety-four senior undergraduates and beginning postgraduate Linguistics students participated in the experiment in fulfilment of a Psycholinguistics course requirement. None of them had participated in Experiment 1. As before, the experiment was conducted prior to class discussion of psycholinguistic studies of constituent structure. Half of the group, 47, acted as subjects and the remaining 47 were observers.

Materials

The eight typed 89-word passages used are reproduced in Figure 5 (overleaf). Each passage contained 8 sentences. The passages were meant to be read as coherent wholes, but each sentence occupied a single line on its own, to avoid any confounding effects from typed lines coming to an end in mid-sentence. Double-line spacing was employed to facilitate reading. Capital letters and full stops were the only punctuation used.

The passages were designed as four pairs, 1 & 2, 3 & 4, 5 & 6, 7 & 8. Within the pairs there are only two differences: (a) the order in which the fifth and sixth sentences appear and (b) the final sentence in one member of the pair was meant to be taken as containing a restrictive relative clause (*The man who ...*) and in the other it was a sentence intended to be taken as containing a non-restrictive relative clause (*Tom Man who ...*). Within a given pair of passages the first seven sentences are identical.

The first four sentences are structurally parallel *across* all four pairs of passages. The fifth, sixth and seventh sentences of each passage were all 13 words in length and they were drawn from a set of: 2 sentences containing ordinary V-PP sequences (VPP_a *speak for a period of several seconds*, VPP_b *stop at the limits of their knowledge*); 3 sentences containing prepositional verbs (PrV_a *contend with*, PrV_b *adjust to*, PrV_c *allow for*); 3 sentences containing phrasal verbs (PhV_a *make out*, PhV_b *find out*, PhV_c *put across*): and two sentences intended to be ambiguous, one of them (Amb_a *... draw up the body of the tongue* = 'raise between a phrasal verb analysis (in passages 1 and 2) and an ordinary V-PP analysis (in passages 3 and 4), and the other (Amb_b *... enlarge on a type of bromium paper* = 'make enlargements on...' or 'say more about...') between a straightforward V-PP analysis (in passages 5 and 6) and a prepositional verb analysis (in passages 7 and 8). The manner in which these 10 sentences were partly counterbalanced across the fifth, sixth and seventh positions of the 8 passages is shown in Table 4.

	Passage Number							
Position	1	2	3	4	5	6	7	8
fifth	Amb_a	VPP_a	Amb_a	PhV_a	Amb_b	PrV_c	Amb_b	VPP_b
sixth	VPP_a	Amb_a	PhV_a	Amb_a	PrV_c	Amb_b	VPP_b	Amb_b
seventh	PrV_a	PrV_a	PrV_b	PrV_b	PhV_b	PhV_b	PhV_c	PhV_c

Table 4 Partial counterbalancing of structures presented as the fifth, sixth and seventh sentences of the 8 passages in Experiment 2. Abbreviations are explained in the text.

1. Traditional British phoneticians study the postures of the speech organs.
One needs to alter the shapes of the articulators in a skilled way to make speech sounds.
The lips must be moved.
We have to touch various positions on the palate.
For velar sounds we have to draw up the body of the tongue.
In normal situations people need to speak for a period of several seconds.
When making observations scientists have to contend with the speed of the performance.
The man who pioneered these studies died last year.

2. Traditional British phoneticians study the postures of the speech organs.
One needs to alter the shapes of the articulators in a skilled way to make speech sounds.
The lips must be moved.
We have to touch various positions on the palate.
In normal situations people need to speak for a period of several seconds.
For velar sounds we have to draw up the body of the tongue.
When making observations scientists have to contend with the speed of the performance.
Tom Man who pioneered these studies died last year.

3. X-ray cine photography reveals the motions of the speech organs.
One needs to mark the surface of an articulator with a barium pencil to make it visible.
The lips must be marked.
We have to make some lines on the palate.
For velar sounds we have to draw up the body of the tongue.
On exposed plates investigators need to make out the shape of each organ.
Before starting measurements subjects have to adjust to the taste of the barium.
The man who pioneered these studies died last year.

4. X-ray cine photography reveals the motions of the speech organs.
One needs to mark the surface of an articulator with a barium pencil to make it visible.
The lips must be marked.
We have to make some lines on the palate.
On exposed plates investigators need to make out the shape of each organ.
For velar sounds we have to draw up the body of the tongue.
Before starting measurements subjects have to adjust to the taste of the barium.
Tom Man who pioneered these studies died last year.

5. Detailed X-ray photographs reveal the nature of the vocal tract.
One needs to increase the size of an image with a convex lens to understand it easily.
The glottis must be widened.
We have to expand the gaps between the articulators.
For various reasons we have to enlarge on a type of bromium paper.
In making measurements investigators need to allow for the blurring of the edges.
By tracing contours phoneticians try to find out the volumes of the cavities.
The man who pioneered these studies died last year.

6. Detailed X-ray photographs reveal the nature of the vocal tract.
One needs to increase the size of an image with a convex lens to understand it easily.
The glottis must be widened.
We have to expand the gaps between the articulators.
In making measurements investigators need to allow for the blurring of the edges.
For various reasons we have to enlarge on a type of bromium paper.
By tracing contours phoneticians try to find out the volumes of the cavities.
Tom Man who pioneered these studies died last year.

7. Many scientific techniques confront the student of the human brain.
One needs to explain the nuances in the methods with great care to make them clear.
The materials must be described.
We have to specify a lot of the details.
For various reasons we have to enlarge on a type of bromium paper.
When studying psychology linguists like to stop at the limits of their knowledge.
After easy introductions teachers have to put across the complications of the subject.
Tom Man who pioneered these studies died last year.

8. Many scientific techniques confront the student of the human brain.
One needs to explain the nuances in the methods with great care to make them clear.
The materials must be described.
We have to specify a lot of the details.
When studying psychology linguists like to stop at the limits of their knowledge.
For various reasons we have to enlarge on a type of bromium paper.
After easy introductions teachers have to put across the complications of the subject.
The man who pioneered these studies died last year.

Figure 5 The eight passages used in Experiment 2. The format of each passage is shown exactly as it was presented to the subjects . The second sentence in passages 7 and 8 has only 16 words. It should have been 17 words long to match the second sentence in the other six passages.

Procedure

Testing took place in four separate sessions (with any given subject attending only one of the sessions) in a language laboratory. Each subject copied one of the eight passages. Assignment of subjects to passages was arbitrary and an attempt was made to ensure that each of the passages was copied by an equal number of subjects at each testing session. The positioning of subjects, observers and pieces of paper was exactly as described for Experiment 1.

After the group had assembled at a testing session, the subjects were withdrawn into a nearby room and subjects and observers were then given separate oral instructions.[11] (This made it easier to answer observers' requests for clarification without alerting subjects to the true nature of the task, than had been the case with the tape recorded instructions in Experiment 1.) Apart from obvious changes relating to the fact that each subject now had only one passage to copy, the instructions were the same for Experiment 1. As soon as they had been briefed, the subjects returned to the language laboratory and testing began.

Results

Table 5 gives the means and standard deviations of the number of pauses per subject for the four pairs of passages.

	Passages			
	1&2	3&4	5&6	7&8
mean	26.6	23.5	23.6	26.6
sd	7.6	7.3	11.9	10.8

Table 5 Means and standard deviations of pauses per subject for the four pairs of passages in Experiment 2. Passage-final pauses are not counted here.

An analysis of variance failed to reveal differences between these four means (F<1, i e, there was more variance amongst subjects copying passages in a given pair than there was from one pair to another pair). This implies that the passages were comparable in level of difficulty, a desirable outcome to have achieved. (Inspection had shown no large differences between the means within each pair of passages.) The overall average length of the sequences that subjects were carrying across before a pause was 3.40 words, which is almost identical with the corresponding mean for the COHERENT 'PHONETICS' passage of Experiment 1 (3.41 words). And, also similarly to the 'PHONETICS' passages, a sprinkling of pauses within words was again recorded. These were treated in the same way as they had been in the analysis of Experiment 1.

As had been found in the first experiment, subjects were strongly inclined to pause at the ends of sentences. Altogether they were recorded as having paused 269 times at full stops (excluding the last one in each passage, where pausing was unavoidable). Out of 329 occasions when they could have done so (47 subjects x 7 sentences), this represents opportunities being taken 82% of the time.

In an experiment such as this one, observer accuracy is obviously a matter for concern and Table 6 presents one check on the quality of observers' records. Some of the sentences were quite long and subjects' written copies of these often extended over more than one line. Was there a tendency for observers to confound instances of subjects lifting their pens to return to the left hand margin with pauses for looking back at the original text? Table 6 classifies every gap between words, except for gaps containing full stops. It turns out that the majority of recorded pauses did not coincide with the ends of written lines. There is essentially no dependency between the two dichotomies (Phi coefficient, $r_\phi=-.098$). This potential distraction, at least, was not confounding the observers.

	Pause recorded	Pause not recorded
Subject ends a line of writing	115	190
Elsewhere	768	2643

Table 6 Relationship between the recording of pauses and whether or not subjects came to the end of a line of writing, in Experiment 2. Sentence-final pauses are excluded here, since subjects often followed the original passages in starting each sentence on a new line. For this table, n = 46 because one subject's written copy was mislaid.

Turning now to individual sentences and sentence types, the results will be presented in the following order: (1) the ambiguous sentences (2) the sentences containing relative clauses (3) the first four sentences of the passages (4) the unambiguous sentences containing prepositional verbs, phrasal verbs and verbs followed by ordinary preposition phrases. The most interesting of these results are those dealt with in (4). The results covered in (3) form a baseline of comparison for (4). On the other hand, (1) and (2) are essentially reports on failures. They are included for the sake of completeness.

(1) The ambiguous sentences

The sequence *draw up* in passages 1 and 2, which were intended to provide a context favouring a phrasal verb analysis, attracted the pattern of pauses *draw*$_2$*up*$_7$*the*, i.e. *draw up* formed a constituent as expected. However, in passages 3 and 4, which were supposed to favour a V-PP analysis, it again yielded a pattern that tended towards the phrasal verb type: *draw*$_3$*up*$_5$*the*. *Enlarge on*, the other ambiguous sequence, gave the pattern *enlarge*$_3$*on*$_3$*a* in passages 5 and 6, where it had been hoped that the context would favour V-PP. This pattern of course fails to connect *on* more closely with either of its neighbours. And in passages 7 and 8, which were supposed to provide pressure for a prepositional verb interpretation, the pattern was *enlarge*$_4$*on*$_0$*a*, which suggests a V-PP analysis. The results recounted in (4), below, establish that the latter is probably the correct analysis for verb phrases having prepositional verbs in them, but that is small comfort.

Inadequately constructed test passages vitiate the comparisons being attempted here. *Post hoc*, the passages were presented to 49 Linguistics students who had not been copiers in either of the experiments. The ambiguous sentence in each passage was underlined. They were asked to read through the passage until they reached the underlined sentence and then write down a paraphrase that represented their "first reaction as to what the sentence means". Each of them read only one passage. Nine of the judges answered in ways that could not be relevantly interpreted, eg. by giving paraphrases that were themselves ambiguous on the crucial issue. Of the remaining forty, 23 judged the *draw up* sentence. Overwhelmingly, it was taken as containing a phrasal verb. Twenty judges paraphrased it this way regardless of which context it had appeared in, and only 3 - all of whom read it in the V-PP favouring contexts - offered V-PP paraphrases. For *enlarge on*, the preferred interpretation was V-PP. All 7 of the judges who read *enlarge on* in the V-PP favouring passages took it this way, but so did 4 of the 10 who had it in passages slanted towards the prepositional verb analysis. Obviously it would have been better to have had these sentences judged by the copiers themselves, immediately after the experiment. Individual judgements and pause patterns could then have been compared. And test passages providing more persuasive contextual pressure would have been desirable.

There is one small indication that the pause distributions for the ambiguous sentences might have been influenced by context: rank correlations[12] comparing the pause distributions of each of these sentences across its two types of context fail to reach significance (for *... draw up ....*, r′=0.46, p>.10; for *... enlarge on ....*, r′=0.41, p>.10). Compared to the rank correlations reported in Table 8 below, for very similar sentences, these non-significant coefficients would be surprisingly low, unless disambiguating contextual pressures were implicated.

(2) Relative clauses

The final sentence in each passage was either *The man who pioneered these studies died last year* (relative clause intended, in the contexts provided by the passages, to be restrictive) or *Tom Man who pioneered these studies died last year* (relative clause intended as non-restrictive). The empirical major constituent break for both sentences turned out not to be after the relative clause, nor before the relative clause, nor in any other reasonably expectable place, but halfway through the relative clause (14 out of 24 subjects paused after *pioneered* in the restrictive version and 14 out of 23 paused after *pioneered* in the non-restrictive version). This puzzling finding will receive further comment in the CONCLUDING DISCUSSION, below. What happened to the left of this strange break might nonetheless be considered to be of interest for the restrictive/non-restrictive distinction. The 24 subjects who copied the putative restrictive version produced the following pattern: $[\underline{\text{The}}_2\underline{\text{man}}_2\underline{\text{who}}_2\underline{\text{pioneered}}]_{14}$. The other 23 subjects gave: $[\underline{\text{Tom}}_5\underline{\text{Man}}]_7[\underline{\text{who}}_5\underline{\text{pioneered}}]_{14}$, for the intended non-restrictive version. It would not be impossible to argue that this difference is rational, with the cluster of 7 pauses occurring where the first comma would standardly be written to demarcate a non-restrictive relative clause. To do so would be opportunistic, however. Notice that the first three interword gaps of the 'restrictive' version contain only 6 pauses and that the corresponding part of the 'non-restrictive' one has 17 pauses. Perhaps the 'non-restrictive' version was somehow peculiar, eg. for having been typed without commas or because *Tom Man* is not a name that 'rings a bell' as a famous phonetician/radiologist, etc. Peculiarity might have led to 'double-take' pausing unrelated to the goals of the experiment.

These two relative-clause sentences yielded pause distributions that are overall very similar. The rank correlation coefficient, r′, is 0.88 (p<.02). The fourth sentence in each passage was the same length as the relative-clause sentences, 9 words, but structurally different (see Figure 5). The amalgamated pause distribution for the 4 fourth-position sentences differs significantly from the combined distribution for the 2 relative-clause sentences (X^2=16.36, df=7, p<.05; and inspection suggests that one slightly undersized `expected' cell entry does not detract from the significance of the result). This difference tends to confirm the text-copying method's sensitivity to constituent structure, even if in the present experiment it has told us hardly anything about relative clauses.

(3) The first four sentences

The average rank correlation coefficients[13] between the pause distributions for each of the first four sentences, across pairs of passages, are displayed in Table 7. The second sentence in passages 7 and 8 was only 16 words long; it should have contained 17 words to match the second sentence in each of the other 6 passages (see Figure 5). The disparity arises after the preposition in the phrase with great care (cf. in a skilled way, with a barium pencil, with a convex lens). From the 11 subjects who copied this maverick sentence, the total number of pauses between with and great was 2. For purposes of analysis, this cluster of 2 pauses was treated as if it had been two single pauses on either side of an imaginary word, i.e. with$_1$**imaginaryword**$_1$great. There is no reason at all to think that this marginal manoeuvre had any noticeable effect on the general tenor of the results.

Sentence position in passages	Sentence position in passages			
	1st	2nd	3rd	4th
1st	.62**			
2nd		.67**		
3rd			.20	
4th				.21

Table 7. Average rank correlation coefficients, $\bar{r}'$, between the pause distributions, across pairs of passages, for each of the first four sentences in the Experiment 2 passages. ** highly significant, $p<.01$. Sentences differ in length. Thus size of coefficient needed for significance varies.

As Table 7 shows, there is a satisfyingly high leel of agreement amongst the pause distributions of the four sentences that appeared in initial position in each passage ($\bar{r}'=0.62, p<.01$); likewise for the set of four sentences in second position ($\bar{r}'=0.67$, $p<.01$). Tree diagrams, with non-terminal nodes intuitively labelled, for these first two sets of sentences are presented in figures 6 and 7. The coefficients for the sentences in third position and in fourth position fail to reach significance, though they do begin to approach that level (.20>p>.10, in both cases). The small measure of agreement amongst the pause distributions for the sentences in third position is almost certainly attributable to the fact that these sentences were too short (5 words long) to attract a reasonable number of pauses from subjects as literate as advanced university students. Overall, these four sentences received only 22 sentence-internal pauses from the 47 subjects, an average of less than half a pause per subject per sentence. A tree diagram based on the amalgamated distribution for these five-word sentences, for what it is worth, was presented as an illustration in the RATIONALE section above (Figure 1).

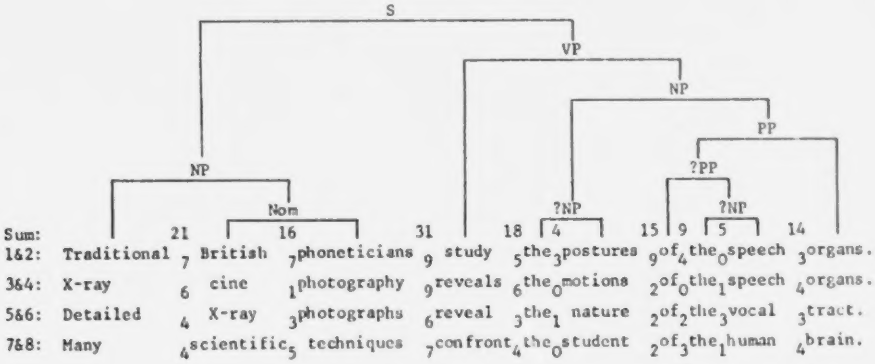
S
VP
NP
PP
?PP
?NP
NP
Nom
?NP
Sum: 21 16 31 18 4 15 9 5 14
1&2: Traditional $_7$ British $_7$phoneticians $_9$ study $_5$the$_3$postures $_9$of$_4$the$_0$speech $_3$organs.
3&4: X-ray $_6$ cine $_1$photography $_9$reveals $_6$the$_0$motions $_2$of$_0$the$_1$speech $_4$organs.
5&6: Detailed $_4$ X-ray $_3$photographs $_6$reveal $_3$the$_1$ nature $_2$of$_2$the$_3$vocal $_3$tract.
7&8: Many $_4$scientific$_5$ techniques $_7$confront$_4$the$_0$student $_2$of$_3$the$_1$human $_4$brain.

Figure 6. Tree diagram for the amalgamated pause distribution of the set of four sentences that appeared in initial position in the Experiment 2 passages. Numbers to the left identify the passages in which they were used.

It is difficult to see why the sentences in fourth position failed to elicit a significant level of agreement - though, at only 9 words long, they do tie with the relative-clause sentences in being the second shortest ones used. A disparity in the numbers of pauses falling between the object NP and the following PP in these sentences does seem explicable, however. Pausing in this gap was as follows: *positions*$_7$*on*, *lines*$_6$*on*, *gaps*$_4$*between* and *lot*$_1$*of*. Unlike the others, the sequence *lot of* is an idiomatic combination, as testified by the possibility of phonological cliticization represented in the non-standard spelling *lotta*. And I assume that idiomaticity is a route which can lead to reanalysis of constituent structure. Omitting the passage 7 and 8 *lotta* sentence from the calculation yields a

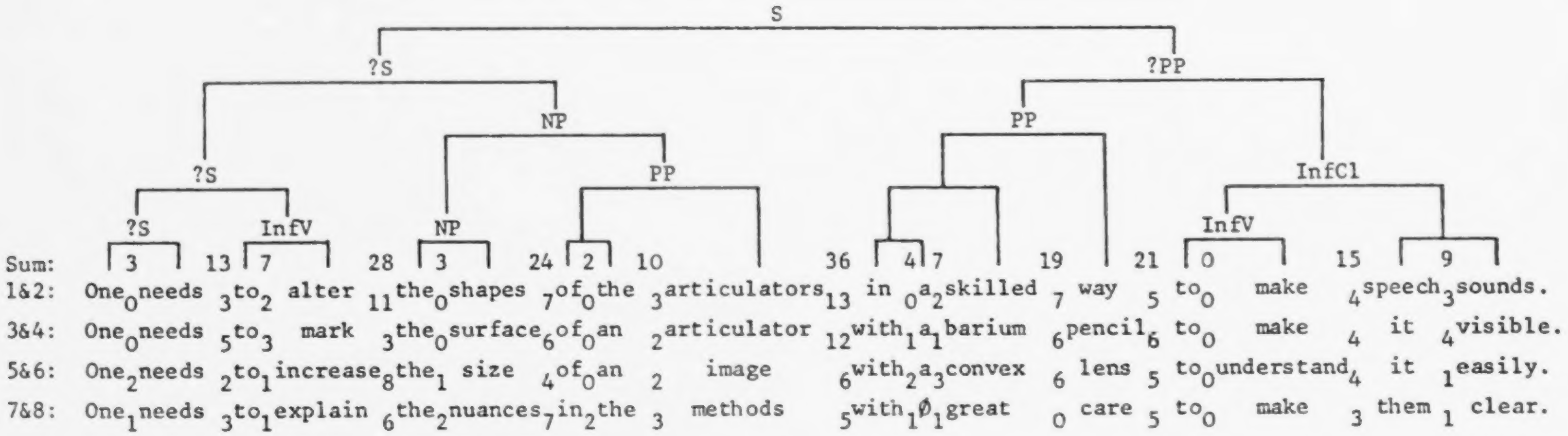
S
?S
?PP
NP
PP
?S
PP
InfCl
?S
InfV
NP
InfV
Sum: 3 13 7 28 3 24 2 10 36 4 7 19 21 0 15 9
1&2: One 0 needs 3 to 2 alter 11 the 0 shapes 7 of 0 the 3 articulators 13 in 0 a 2 skilled 7 way 5 to 0 make 4 speech 3 sounds.
3&4: One 0 needs 5 to 3 mark 3 the 0 surface 6 of 0 an 2 articulator 12 with 1 a 1 barium 6 pencil 6 to 0 make 4 it 4 visible.
5&6: One 2 needs 2 to 1 increase 8 the 1 size 4 of 0 an 2 image 6 with 2 a 3 convex 6 lens 5 to 0 understand 4 it 1 easily.
7&8: One 1 needs 3 to 1 explain 6 the 2 nuances 7 in 2 the 3 methods 5 with 1 0 1 great 0 care 5 to 0 make 3 them 1 clear.

Figure 7. Tree diagram for the amalgamated pause distribution of the set of four sentences that appeared in second position in the Experiment 2 passages. Numbers to the left identify the passages in which they were used.

higher average rank correlation for the other three fourth-position sentences; one which now definitely approaches significance ($\bar{r}'=0.48$, $.10>p>.05$). The empirical constituent structure of these sentences is postponed for examination in the CONCLUDING DISCUSSION below (see Figure 9).

(4) Unambiguous sentences with prepositional verbs, phrasal verbs and ordinary V-PP sequences

These sentences, which appeared in various orders and combinations (together with the ambiguous sentences) in fifth, sixth and seventh positions in the passages (see Table 4), produced the clearest and most encouraging results of Experiment 2. Table 8 presents the relevant rank correlations and X^2 comparisons between the three types of sentence. The coefficients on the main diagonal show that within the sets of sentences representing each of the three types there was a highly significant level of agreement. The other statistics show that the amalgamated pause distribution for the sentences with phrasal verbs was very clearly different from those for the other two types of sentence (see the X^2s in the bottom row of Table 8), while the amalgamated distribution for sentences with prepositional verbs was essentially indistinguishable from the one for sentences with ordinary V-PP sequences (a highly significant rank correlation and a completely non-significant X^2 value, $.90>p>.80$). Regarding the highly significant X^2s in the bottom row of Table 8, inspection reveals that by far the largest component of X^2 in each case arises out of the verbs of phrasal verbs being in close

	Type of structure		
Type of structure	VPP (2 sentences)	PrV (3 sentences)	PhV (3 sentences)
VPP	$r'=.79^{**}$		
PrV	$r'=.73^{**}$ $X^2=2.50$ns	$\bar{r}'=.72^{**}$	
PhV	$X^2=20.95^{**}$	$X^2=34.12^{**}$	$\bar{r}'=.73^{**}$

Table 8. Correlations and contrasts within and between the pause distributions for the unambiguous Experiment 2 sentences containing verbs followed by ordinary preposition phrase adjuncts (VPP), prepositional verbs (PrV) and phrasal verbs (PhV). ** highly significant, $p<.01$.

construction with the particles that immediately follow them, while in both of the other types of sentence the verbs are relatively separated from the following prepositions. A sizeable secondary X^2 component, in each case, comes from the particles of phrasal verbs being relatively disjointed from the object NP, whereas the prepositions in the other two types are closely associated with the NP which follows. Nearly all of the other components of these two X^2s are negligible in size.[14] The highly significant differences between whole sentences are therefore almost entirely attributable to the difference in backeting focused on in the design of this part of the experiment, viz: [[V PARTICLE]NP], for phrasal verbs, as opposed to V[PREP NP]. Tree diagrams for the three sets of sentences discussed here appear in Figure 8 (opposite). As before, the node labels have been added on an intuitive basis.

Grosjean _et al_ (1979:74 & 76) report the same structure as above for the phrasal verbs _call up_ and _bring out_ in their study of pause durations in reading aloud. However, their method also gives this phrasal-verb-type bracketing to a prepositional verb that they tested (1979: 71): ...[[_wondered about_] _this_] [_extraordinary story_]. They attribute this result to the fact that _about this extraordinary story_ is longer than _wondered_: "... pauses will coincide with this break [i e the boundary immediately after the verb] only if the verb and the following NP (or PP) are of equal length..." (1979: 74). Comparison on this point suggests that text-copying may be a better technique for revealing linguistic constituent structure. The three sentences providing the data for the middle (prepositional verb) diagram of Figure 8 have six-word PPs following the verb, but in all three cases the break immediately after the verb is marked by more pauses than fell at any of the breaks within the PP.

CONCLUDING DISCUSSION

Particular constructions

While there certainly are aspects of Figures 3, 6, 7, 8 and (see below) 9 that do not harmonize tidily with any distillation of theoretical linguists' descriptions of comparable sentences, there are numerous points of agreement as well. The large number of nodes in the tree diagrams that it has been straightforwardly possible to label support this claim. Sentence intiial adverbials, sentence subjects and preposition phrases provide further support, though there are important qualifications to be mentioned in connection with the latter two.

In the two experiments, subjects saw a total of 13 sentences with initial adverbials (S_2, P_2 and P_3 in Experiment 1, and the Experiment 2 sentences in 5th, 6th and 7th positions; each of the ambiguous sentences being treated here as a single sentence with its pause distribution being the sum across its different contexts). These adverbials were of various kinds: preposition phrases and finite and non-finite clauses. In 11 of the 13 sentences the pause distributions yielded bracketings in which the adverbial was a constituent, and furthermore an immediate constituent of

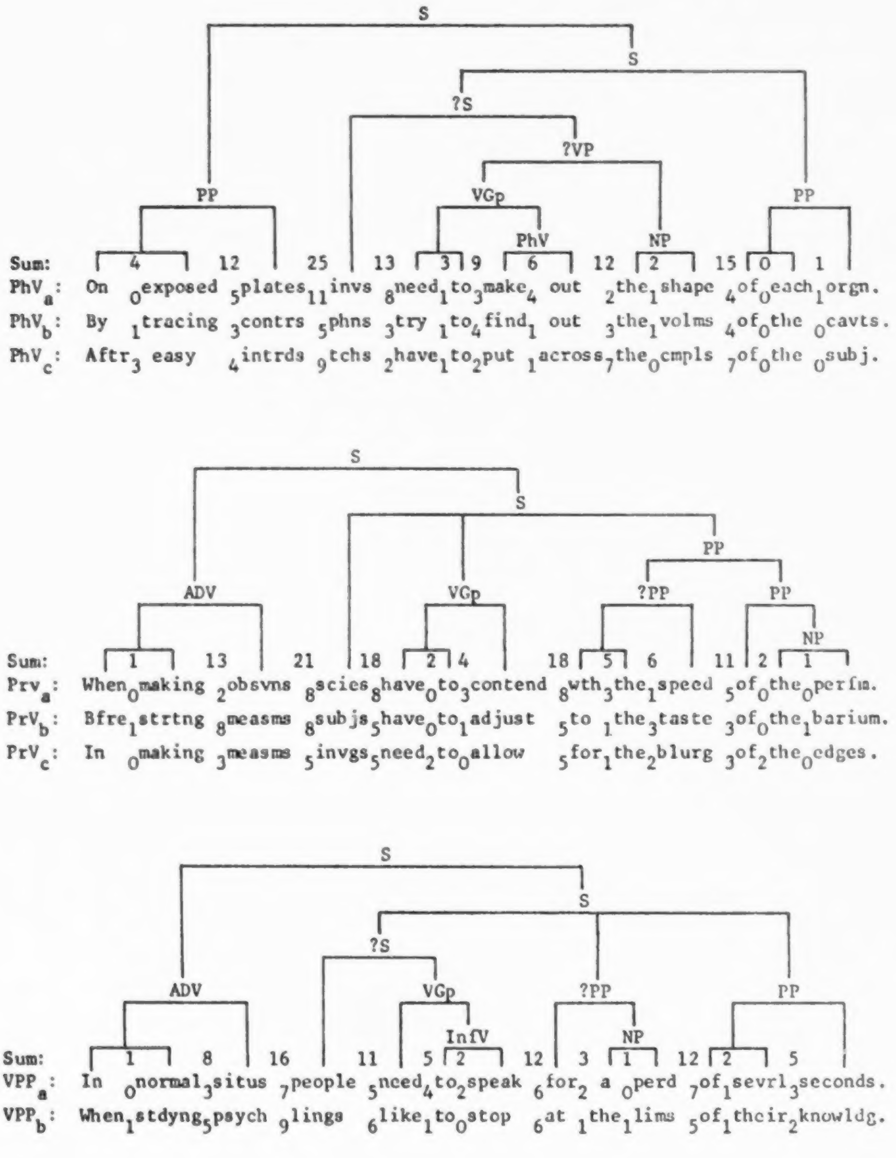
S
S
?S
?VP
PP
VGp
PhV
NP
PP
Sum: 4 12 25 13 3 9 6 12 2 15 0 1
PhVa: On 0exposed 5plates 11invs 8need 1to 3make 4 out 2the 1shape 4of 0each 1orgn.
PhVb: By 1tracing 3contrs 5phns 3try 1to 4find 1 out 3the 1volms 4of 0the 0cavts.
PhVc: Aftr 3 easy 4intrds 9tchs 2have 1to 2put 1across 7the 0cmpls 7of 0the 0subj.
S
S
PP
ADV
VGp
?PP
PP
NP
Sum: 1 13 21 18 2 4 18 5 6 11 2 1
Prva: When 0making 2obsvns 8scies 8have 0to 3contend 8wth 3the 1speed 5of 0the 0perfm.
PrVb: Bfre 1strtng 8measms 8subjs 5have 0to 1adjust 5to 1the 3taste 3of 0the 1barium.
PrVc: In 0making 3measms 5invgs 5need 2to 0allow 5for 1the 2blurg 3of 2the 0edges.
S
S
?S
ADV
VGp
?PP
PP
InfV
NP
Sum: 1 8 16 11 5 2 12 3 1 12 2 5
VPPa: In 0normal 3situs 7people 5need 4to 2speak 6for 2 a 0perd 7of 1sevrl 3seconds.
VPPb: When 1stdyng 5psych 9lings 6like 1to 0stop 6at 1the 1lims 5of 1their 2knowldg.

Figure 8. Tree diagrams for the Experiment 2 amalgamated pause distributions of the unambiguous sentences with phrasal verbs (top), prepositional verbs (centre) and verb followed by ordinary preposition phrase (lower diagram) structures. Some long words have been contracted. See Figure 5 for full versions of the sentences.

the whole sentence. For 7 of these 11 positive cases the correspondence with theoretical linguists' accounts is even closer, the emirical structure being $_S$[ADV[S]], i e the break after ADV is the major constituent break of the sentence.

It is a commonplace of introductory syntax classes that some students balk at accepting that ordinary transitive English sentences split naturally into two parts: a subject noun phrase and a predicate phrase (VP). And distributional arguments for this constituency woo them only reluctantly away from such alternative analyses as [[Subject+verb]Object]. This latter, unorthodox, bracketing has sometimes received a modicum of support from other empirical approaches to constituent structure, e g Martin (1970), Levelt (1970:121) and Grosjean *et al* (1979: 72). However, my pause data offer good evidence in favour of the distributionally justified analysis, *except when the sentence subject is a pronoun*. Ignoring clauses with pronominal subjects, there were in the two experiments 27 clause subjects, which presented the copiers with a total of 245 occasions on which they could pause immediately after copying a subject NP. In 149 instances they did opt to pause. That is to say that the constituent break after a full NP subject was respected 61% of the time, which approaches the figures of 82% and 86% of opportunities taken for pausing at the ends of sentences, as reported in earlier sections of this paper.

For the 17 clauses with pronominal subjects (*it, this, one, we* and the relative pronouns *who* and *which*) the result is quite different. This set of clauses also provided the copiers with a total of 245 opportunities for pausing immediately after the subject, yet they did so only 23 times, 9%. This behaviour in relation to pronominal subjects may be an artefact of the experimental method used, given that pronouns are short function words and that in 10 cases out of the 17 they were also the first word of a sentence (which means that they appeared in the position where the text-copying method shows least sensitivity). However, the number of languages in which subject pronouns are optional or are clitics on the verb suggests that it would be imprudent to dismiss out of hand this possible pointer to pronominal subjects being similarly different from full NP subjects in English too.

Altogether there were 53 preposition phrases in the test passages of the two experiments. (Here the preposition of a prepositional verb is counted as starting a preposition phrase, in line with the empirical findings reported above. Phrasal verb particles are, of course, not treated as prepositions. Again, each of the ambiguous sentences was regarded as a single sentence, notwithstanding its different contexts.) These preposition phrases emerged clearly as unified constituents: for 43 of the 53 (i e 81%)

there were fewer pauses at any point within the phrase than in either of the gaps flanking it.

An interesting comparison is possible here with some data published by Hawkins (1971) on hesitation pauses in a sample of children's spontaneous narrative speech. Of the 53 preposition phrases in the present test material, there were 39 that contained a determiner (e g *between the articulators*, *with a convex lens*, as opposed to e g *of bromium paper*, *on exposed plates*) and all but one of these 39 appeared in either final position or mediallyin their sentences. For the 38 non-initial preposition phrases containing determiners the overall amalgamated pause pattern was: $_{201}\mathrm{PREP}_{46}\mathrm{DET}_{53}\mathrm{N}$ or ADJ. Summing 201, 46 and 53 gives a total of 300 pauses adjacent to the prepositions and determiners. The pause pattern expressed in percentages of 300 is then $_{67\%}\mathrm{PREP}_{15.3\%}\mathrm{DET}_{17.7\%}\mathrm{N}$ or ADJ, and this is remarkably close to the corresponding figures derived from the last line of Hawkins' Table 1 (1971:283): $69.5\%^{\mathrm{PREP}}13.3\%^{\mathrm{DET}}17.1\%^{\mathrm{N}}$. Notice that both sets of data would then seem to license the curious bracketing $_{pp}$[[PREP DET]...]. However, this probably gives too much weight to the small different between 46 and 53 pauses (for Hawkins' data the raw figures are 14 and 18 pauses, respectively). Furthermore, when the 38 non-initial determiner-containing preposition phrases are examined individually it is found that, although 20 of them attracted fewer pauses to the gap preceding the determiner than fell immediately after it (i e [[PREP DET]....]) and only 11 had the converse pattern of more pauses before the determiner than after it (i e [PREP[DET....]]), this difference fails to be statistically significant (two-tailed sign test corrected for continuity, p=0.19). It therefore seems safest to conclude that, while the present results amply confirm the reality of preposition phrases as constituents, they do not reliably map the internal structure of preposition phrases.

Though experimental 'noise' (discussed further at the end of this paper; see *Final remarks*) masks some of the details and makes for some untidiness, the points listed above about particular constructions suggest that there is a fair measure of agreement between the empirical pause distributions and expectations derived from theoretical linguistics. What light then do the empirical results throw on structures for which there is not yet a consensus among theoretical linguists? One hoped-for clarification failed to materialize: for reasons already discussed, the Experiment 2 results do not firmly demonstrate any differences between sentences with restrictive relative clauses and those with non-restrictive relative clauses (which is not to say that there are no differences; just that the materials employed here were not good enough for the task of finding whatever differences may exist). Note, however, that both of the Experiment 1 sentences containing relative clauses, S_4 and P_1, yield the same pattern - and one that is compatible with a popular analysis of restrictive relative clause structures - viz. $_{NP}[[NP]WH....]$ (see Figure 3).

More positively, Experiment 2 gave an unequivocal verdict on the constituency of the prepositional verbs tested. Their structure is no different from that found in ordinary V-PP sequences, and it is sharply different from transitive phrasal verb constructions. No clear generalizations about the relationship of preposition phrases to other constituents emerge in the present results. However, it is interesting that the *by*-phrases in the two passive sentences tested (S_2 and P_3: see Figure 3) both turn up as 'daughters' of VP. Also, preposition phrases which act as nominal modifiers do several times, though not always, appear in close construction with the modified NP, e g S_1 and P_4 in Figure 3, and the tree diagram in Figure 6.

When a sentence contained one or more auxiliary verbs, a constituent comprising just the auxiliarie(s) and main verb generally came to light. This may be taken as support for the Hallidayan *verbal group*, and such constituents have accordingly been labelled *VGp* in Figures 3 and 8. See Halliday (1976) and Huddleston (1976:81 & ch14) for some relevant theoretical linguistic discussion of this matter. Martin (1970) found a strong predilection for this grouping and Grosjean *et al* (1979: 78) apparently observed a similar tendency too. An additional observation on constituency in which verbs participate concerns the infinitival marker *to*. The passages for Experiment 2 contained 22 instances of this *to* (as before, each of the ambiguous sentences is treated as a single sentence). In 14 of these cases *to* turned out empirically to be in close construction with what followed it, usually giving a constituent [*to* V], and in 6 cases *to* joined on to what preceded, usually forming a constituent [V *to*]. The remaining two examples had the empirical constituency [...V *to* V]. Although the number of pauses involved is small and the difference between the number of pauses before and after *to* was often slight, the preponderance in favour of the [*to* V (....)] bracketing is statistically significant (two-tailed Wilcoxon T-test, $p<.05$). Six of the seven instances in which *need(s)* was the first verb conformed to this pattern.

Comprehension in text-copying

Immediately after the end of Experiment 2, the subjects were asked to write down any general comments they had and also to give a subjective impression of how well they had understood their test passage: "Did you understand what you were writing while you were doing it?" Six offered general remarks but neglected to comment on their understanding of the passage. That leaves 41 who did make notes relevant to this question. Only 13 of these 41 answers were negative, but even so the majority of negative answers were tempered

("I was not really understanding it as I wrote", "I didn't feel I was really taking it in", "I didn't understand what I was copying. I knew vaguely that it was about phonetics", etc). Only 2 of the 13 negative responses lacked this sort of qualification and they both said that they "would not have been able to answer a comprehension test" and one of them added "because I was writing quickly, without bothering to try and remember". Since memory for the purpose of answering a comprehension test would be something over and above mere comprehension at the time of copying, it is open to us to believe that even these two subjects perhaps understood a little of what they were copying. Varying degrees of positive comprehension were claimed by the remaining 28 subjects (i e by 68% of the 41 who answered the question). According to their own reports, therefore, many - perhaps nearly all of the copiers (remember the qualifications embedded in the majority of negative responses) - showed at least some understanding of the passages, which is to say that the task was apparently one that, possibly to varying extents, did engage high level linguistic analyzing mechanisms.

On the other hand, only 5 of the 41 subjects made unqualified affirmative claims regarding their comprehension ("I understood what I was writing", "The material was easy to understand while I was copying", "I understood the passage", etc). If it is accepted that the Experiment 2 passages were not so contrived and to be nearly incomprehensible, it is worth asking why so many subjects found cause to dilute the strength of their affirmations ("The last sentence was the only one I properly understood. The others I understood fragments of", "In most cases I think I understood", "I didn't get the whole thing but, yes, a part of it", "Vaguely understood the passage", etc). For 6 subjects the issue is fairly clear: they reported comprehension of the individual sentences but failure to integrate the sentences into a coherent whole ("Had some idea of what I was writing about, but limited to the sentence rather than the overall structure of the whole passage", "I understood the text whilst copying, but [....] didn't relate the content of the various sentences", etc).

These 6, plus 5 unqualified affirmatives, still leave unaccounted for 30 out of 41 (or 28 out of 41 if the two who doubted that they would succeed in a comprehension test are presumed to have understood nothing) who somehow felt that they had not properly grasped the meaning of what they copied, notwithstanding the fact that they were intelligent, highly-literate, experienced readers. Why? Perhaps because their language analyzing mechanisms were only partly engaged. If so, it behoves us to ask which parts were in play and which were idling. The tree diagrams in Figure 9 suggest a possible answer to this question. They represent the amalgamated pause distribution for the Experiment 2 relative-clause sentences and that for the set of four sentences which appeard in fourth position in the Experiment 2 passages. Amalgamating the pause distributions for the fourth-position sentences and representing them by means of a single tree diagram is a questionable move in view of the low correlations amongst these four sentences ($\bar{r}'$=0.21, as shown in Table 7), but the aspects of this tree commented upon below are not seriously affected by disparities within this set of four distributions.

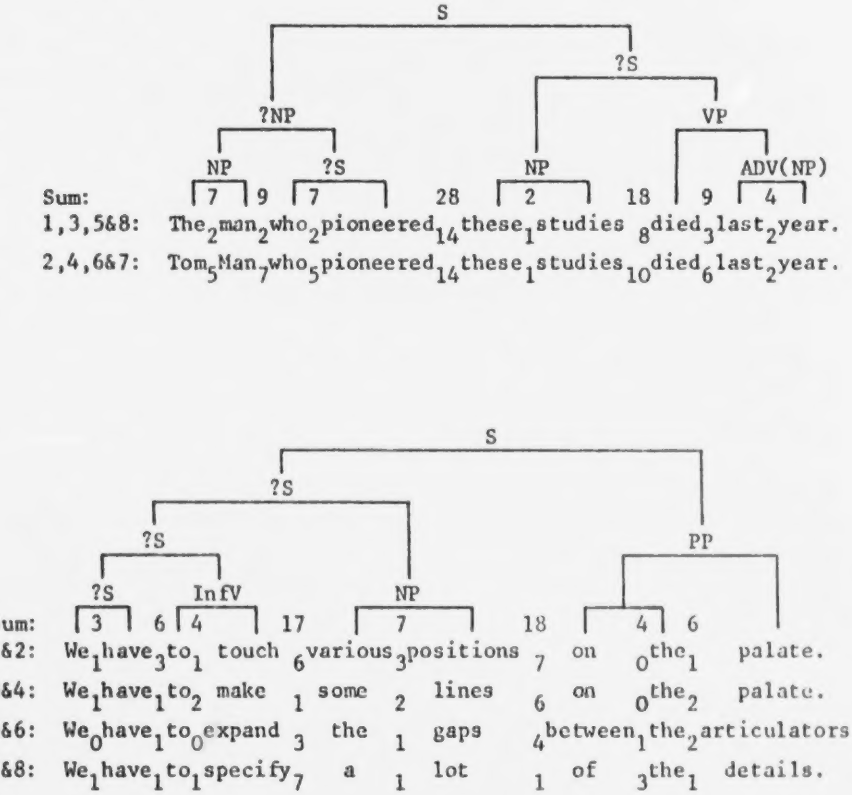
S
?S
?NP
VP
NP
?S
NP
ADV(NP)
Sum: 7 9 7 28 2 18 9 4
1,3,5&8: The₂man₂who₂pioneered₁₄these₁studies ₈died₃last₂year.
2,4,6&7: Tom₅Man₇who₅pioneered₁₄these₁studies₁₀died₆last₂year.
S
?S
?S
PP
?S
InfV
NP
Sum: 3 6 4 17 7 18 4 6
1&2: We₁have₃to₁ touch ₆various₃positions ₇ on ₀the₁ palate.
3&4: We₁have₁to₂ make ₁ some ₂ lines ₆ on ₀the₂ palate.
5&6: We₀have₁to₀expand ₃ the ₁ gaps ₄between₁the₂articulators.
7&8: We₁have₁to₁specify₇ a ₁ lot ₁ of ₃the₁ details.

Figure 9. Tree diagrams for the amalgamated pause distributions of the Experiment 2 sentences containing relative clauses and for the set of four sentences which appeared in fourth position in the passages. Numbers to the left identify the passages in which they were used.

The tree diagrams of Figure 9 suggest that some subjects were 'jumping to confusions' about constituency at the earliest possible opportunity. *The man who pioneered* could be a subject NP (it would be in a sentence such as *The man who pioneered had to be self-reliant*). Someone reading the passage for sense would discover from the sequel, *these studies died last year*, that there was more to the subject

NP: that *these studies* was the object of the verb in its relative clause. But someone who merely had to transfer a convenient chunk of text into memory for the purpose of making a copy might well be satisfied to grab the first string that *could possibly* form a constituent, without studying the remainder of the sentence to check that it was in fact a constituent of the sentence in hand. This is perhaps what was being alluded to by the following two answers to the post-test question about comprehension: "I did understand the sentences, but only after I'd written them in their entirety" and "I waited and checked what I was copying because I wanted to make some sense out of it". Obiously comprehension will be fragmentary if one takes *The man who pioneered* to be a subject NP that has somehow lost its predicate and then reads the string *these studies died last year* as a separate sentence in its own right.

Likewise in the lower diagram of Figure 9, *We have to touch* is a *possible* constituent (in a different context it could be a full clause) and so is *We have to touch various positions*, and perhaps even the fragment *We have*. But they - and the corresponding fragments of the other three sentences - are strings whose adoption as constituents would confound attempts to extract the full meaning of the particular sentences containing them here. Inspection of other tree diagrams presented earlier in this paper reveals further instances of this phenomenon, for instance the 'false' S constituents in the left half of Figure 7 and the 'false' adverbial *Before starting* in Figure 8.

For text-copying as an empirical technique to reveal the constituent structure of sentences, these observations have nuisance value (which could presumably be abated by careful preliminary editing of the test passages and by telling copiers that they will be asked comprehension questions afterwards). However, they are more interesting when viewed as clues to the nature of human on-line linguistic analysis. Kimball (1973) put forward several hypotheses as to how language users do surface structure parsing. One of these, his *Principle 5*, is clearly applicable here: "a phrase is closed as soon as possible, i e, unless the next node parsed is an immediate constituent of that phrase". The present results suggest that Kimball's Principle 5 can be decomposed into two subprocesses: 'close a phrase as soon as possible' and 'check that the next node parsed is not an immediate constituent of the last phrase thus closed'. The decomposability has come to light because text-copying does not demand full comprehension: easily memorizable constituents can be extracted from a text even by a subject ignoring the *unless*-clause of Principle 5. This interpretation is obviously also compatible with Frazier & Fodor's (1978) two-stage parsing model. I surmise that the kind of minimal comprehension required for efficient text-copying provides a laboratory example of the embarrassment of the tired reader who comes to the foot of a page and then has to return to the top to re-read the page for a proper understanding of it.

There is an additional datum in the comprehension results which, though it certainly does not reach statistical significance here, would be worth investigating further. This is that 3 of the 5 copiers who made unhedged claims to the effect that they had understood what they were writing out all paused less often than the median number of pauses per copier and, by contrast, 8 of the 13 whose statements about their comprehension were negative had individual pause totals greater than the median.

Final remarks

For the 70 copiers in the two experiments there were 70 different observers. The observers were furthermore untrained, their task (but not the hypotheses) having been explained to them only minutes before they began making observations. This arrangement had the desirable consequence of rendering it extremely unlikely that the constituents which emerged in the aggregated pause distributions were the products of a wishful experimenter consciously or unconsciously imposing a favourite theoretical linguistic analysis in the process of deciding what to record as glancing-back-to-the-original-passage pauses. But the pooled observations of 70 inexperienced observers might easily have been uninterpretably chaotic. Undoubtedly, variations between observers and the fact that they had only a single quick chance to observe each pause must have generated considerable `noise' in the data. The extent to which intuitively reasonable structures nonetheless penetrated the noise is all the more impressive. It testifies both to the salience of syntactic constituents as coding units in linguistic processing and to the robustness of the test-copying method. It should be noted that this noise is not indissociably linked to the method: cleaner data could doubtlessly be obtained by videotaping the copiers and having a limited number of judges code the tapes according to an agreed scheme. There are, however, two biases in the text-copying results: as mentioned earlier, the constituency of the beginnings of sentences probably tends to be blurred on account of relatively few pauses being attracted there, and very short constituents are probably not given due prominence. One approach to these problems would be to simply ignore those parts of the constituent bracketings where the biases are *a priori* likely to have operated. Another solution would be to attempt to estimate the magnitude of the biases and to weight the pause distributions accordingly.

Footnotes

1 Various parts of this work have formed the basis of talks given in a number of universities in Britain and India, as well as a short paper presented to the 1982 Summer Meeting of the Linguistic Society of America. I am grateful for audience comment, which has been most helpful on all occasions.

2 I am inclined to add to the desiderata of naturalness of task, simultaneity and sensitivity: that an ideal psycholinguistic method for the elucidation of constituent structure should also be economical in the use it makes of subjects' and experimenters' time. Some may feel that economy is an extraneous consideration in 'pure science', but an uneconomical technique is unlikely to be used outside of feasibility demonstrations, whereas one which easily gives information on many sentences stands a chance of becoming a real tool of discovery. Of the methods mentioned in the text, Levelt's, Johnson's and the click approach seem to me to be uneconomical ones. For instance, for a seven-word sentence (which might be uttered in under two seconds) Levelt would require considered judgements on 21 pairs of words from each subject. The pause studies, to be discussed next, make better use of subjects' time, since a small corpus of recorded speech will contain large quantities of data (though analysis is bound to be time-consuming).

3 The idea of the text-copying method was suggested to me by a technique which Chase & Simon (1973) used to investigate chess playing.

4 Grosjean & Lane (1977) independently devised what amounts to the same algorithm.

5 Height in the tree could be scaled to reflect the number of copiers pausing at each boundary, but I have chosen not to do so in this article. Such a scaling has no obvious theoretical linguistic interpretation, though it might be a useful graphic aid for suggesting whether or not a given bracketing is reliably distinct from alternative possibilities.

6 The data from this set of five-word sentences provide a convenient illustration of the algorithm, but it will be noted in the account given later of Experiment 2 that the small number of sentence-internal pauses involved makes this particular result an unreliable one.

7 The line of argument presented in the body of the paper gives the benefit of doubt to those who would

posit word-length as the determining factor responsible fcr the Figure 3 results. One could, conversely, argue that pauses should be expected immediately *after* short words: subjects should stop reading and empty their short-term memory contents via their pens before tackling a long word; then, having subsequently absorbed the long word, they will often find that they still have room for one or more short words. This strategy would yield tree diagrams that mostly violated theoretical linguistic accounts of English sentence structure.

8 See Vestergaard (1977) for a monograph-length study on this topic and closely related matters.

9 For some discussion of alternative anlyses of relative clauses, see Stockwell, Schachter & Partee (1973: ch 7) and, especially, Partee (1976: 53f).

10 An unanticipated third interpretation, 'draft (a picture of) the body of the tongue', was offered by some of the judges who (see below) were subsequently asked to paraphrase the ambiguous sentences. This reading is also presumably related to a phrasal verb analysis of *draw up*.

11 I am grateful to John Local and Kit-Ken Loke for assistance in conducting Experiment 2.

12 X^2 'goodness of fit' comparisons are rendered infeasible here by the numbers of pauses in some of the relevant loci being too small.

13 Here, and where they are reported elsewhere below, average rank coefficients, $\bar{r}'$, were derived from coefficients of concordance corrected for continuity and, when necessary, for ties.

14 It should be pointed out that the small numbers of pauses which fell in some of the inter-word gaps made it necessary to combine some adjacent clusters of pauses for the purpose of calculating the X^2 values presented in Table 8. This reduced the degrees of freedom to 6, from a potential 11. The sequences within which pauses were combined and treated as if they had occurred in a single location were the same for all three types of sentence: the initial AdvP, the string Aux-to-V and the sentence-final PP-modified NP (an example of the latter being *the shape of each organ*). Even so, one of the 'expected' frequencies was an uncomfortably low 3.8. This slightly exaggerates the resulting X^2 value, but as it occurs in the comparison between PrV and VPP - the non-significant X^2 - it simply adds force to the argument that the PrV and VPP sentences have the same structure.

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GAPS IN GENERALISED PHRASE STRUCTURE GRAMMAR

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Proponents of Generalised Phrase Structure Grammar (GPSG) have, since its inception , made claims for the superiority of the analyses which the theory makes available for certain problematical constructions of English. In this note I will consider two particular examples:

a. Rightward Unbounded Dependencies (including Right Node Raising)

b. Parasitic Gaps

My intention will be to show that, as GPSG has evolved, various steps taken in its development with a view to providing explanatorily adequate accounts of central properties of English and other languages have rendered previous claims in respect of these two types of construction invalid - to the extent that there is no obvious way in which the resulting paradoxes can be resolved in the current theory.

Rightward Unbounded Dependencies (RUDs)

Gazdar (1981a, 1981b) sketches an analysis of RUDs intended to account for, examples of 'extraposition from NP' like (1)

1. The woman believed the man was ill who was here

He proposes that these sentences are invoked by the following rule

2. $\alpha \dashrightarrow \alpha/\beta \quad \beta$

where α ranges over clausal categories and β can be any phrasal or clausal category

This rule interacts with general principles for distributing slash categories to assign the following structure to sentences like (1) (slightly simplified)

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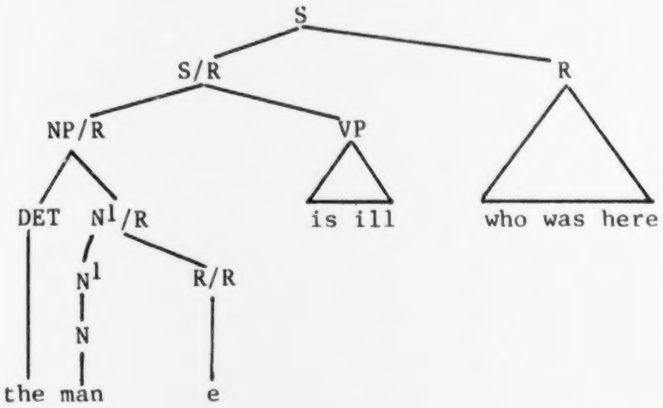
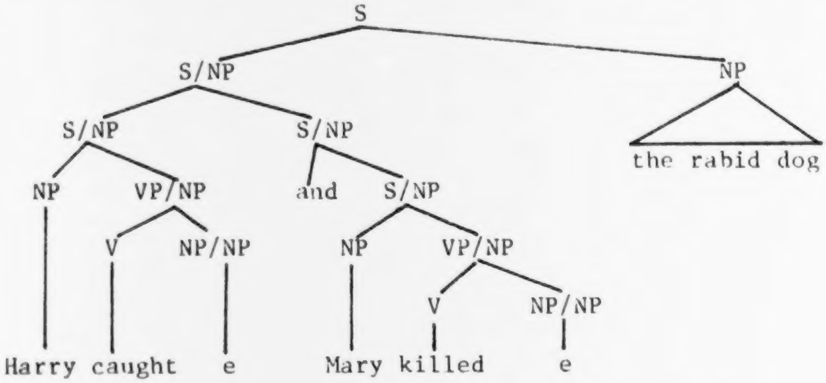
3.



Gazdar claims further that rule 2, together with principles determining the distribution of slash categories, and the GPSG analysis of coordination together provide a successful account of Right Node Raising (RNR).

Analyses of coordination in GPSG impose a certain degree of identity on the feature composition of conjuncts in the structure. For our purposes, the relevant consideration is that if a slash feature is instantiated on a conjunct, the same instantiation is required on all the other conjuncts in the construction, and on the mother. This requirement, together with the rule in (2) will ensure that the grammar of English generates structures like (4)

4.



This is a striking result, since, as Gazdar points out, quoting Jackendoff (1977), 'there are no remotely coherent formulations of RNR'.

My claim here is that, given recent developments, GPSG itself now has no coherent analysis of RNR (or other RUDs) either. The two aspects of recent GPSG which lead to this conclusion are the following

a. the Lexical Head Constraint on metarule application

b. the role of the Head Feature Convention in constraining the instantiation of slash features

The Lexical Head Constraint (LHC)

The LHC is concisely stated in GKPS (59) as:

> 'metarules map from lexical ID rules to lexical ID rules'

Lexical ID rules are defined (54) as follows,

> 'A rule is lexical only if it has a head which is an extension of a SUBCAT category.'

(Categories defined for the feature SUBCAT are effectively those which immediately dominate lexical items.)

What this entails is that only those rules which have a lexical head can serve as the input and output of metarules. This is relevant to the examples we are considering because (for reasons given in Sag, 1982) the rules which terminate unbounded dependencies (Slash Termination Metarules, STMs) are (necessarily?) introduced by metarules. The LHC thus ensures that UDs must terminate in a subtree that contains a lexical daughter (a condition which has similar effects to part of the Empty Category Principle in Government Binding Theory, as Horrocks, 1984, points out).

The imposition of the LHC has at least two different motivations

> a. it imposes constraints on the strong generative capacity of GPSG
>
> b. in the case of UDs, it provides a explanation for a number of empirical observations concerning the impossibility of extraction from various positions.

It accounts, for example, for the ungrammaticality of the following (taken from Flickinger, 1981, which should be consulted for further discussion of the motivation for the LHC)

5. *Who did you say that ___went?

6. *Whose did John borrow ___ book?

7. *By whom did John think (that) Bill lost six books ___?

8. *Afraid of heights, we kept every child ____ inside.

These sentences will not be admitted by a grammar respecting the LHC since none of the rules required for the termination of the UD are lexical:

(5) involves either (5.a) or (5.b)

5.a. S --> NP, H[-SUBJ][1]

5.b. S[COMP that] --> {[SUBCAT that]}, H[NIL]

((5) is relevant if Slash Termination Metarule 1 (STM1) (GKPS:143) is invoked, (5b) if Slash Termination Metarule 2 (STM2) is (GKPS:161). Note that, even though (5.b) contains a SUBCAT category, this category is not the head of the rule, and the rule therefore fails to satisfy the LHC)

(6) involves

6.a. NP --> NP, H[BAR 1]

(7) involves

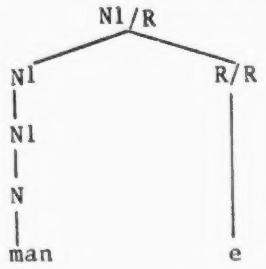
7.a. N1 --> H, PP[by]

and (8) involves

8.a. N1 --> H, AP

Note now that Gazdar's analysis of (1) requires a violation of the LHC. The relevant subtree is

9.



which assumes the analysis of relative clauses given in GKPS (155), in which they are analysed as sisters of N1:

9.a N1 --> H, S[+R]

Since the head in this rule is not defined for SUBCAT is not within the domain of the STMs and (9) will not be admitted by the grammar.

A second example, involving RNR, also comes from Gazdar (1981b), after Bresnan (1974),

10. I've been wondering whether ____, but I wouldn't positively like to state that ____, your theory is correct.

Here, the subtree required at the extraction site is (11)

11. S/S

[{SUBCAT whether}] S[+NULL]/S

Observe that the input ID rule required for this tree is (5b), modulo the different value for SUBCAT. However, to handle leftward UDs correctly and to get the 'that-trace' facts exemplified in (5) right, this rule must not be a possible input to the STMs, whereas, to get (10), it must. This example presents the paradox in its sharpest form.

A final example makes the same point:

12. I think that Max ___, and I know that Oscar ___, will be going to the party.

Here, the rule required for slash termination (presumably by STM2 in this case) is

13. S --> NP, H[-SUBJ]

This also is not a lexical rule.[2]

In the face of these problems, one might consider tinkering with the definition of Lexical Rule, and reformulating the way GPSG should handle the 'that-trace' phenomenon. There are however further problems associated with the GPSG analysis of these constructions which preclude a solution along these lines, to which I turn next.

Slash Feature Propagation

In GKPS, the propagation of slash features is effected by feature instantiation, constrained be the HFC and FFP. A local tree is admitted by a rule only if every category in the tree is a legitimate extension of the corresponding category in the rule. SLASH is stipulated to be a member of the set of HEAD features. This means that when slash features are instantiated, the mother and the head of the rule must bear identical slash feature specifications. (Unless the head is a SUBCAT category, in which case at least one of the non-lexical daughters must be extended which the same slash feature specification as the mother.) It is this requirement, that instantiated slash features must occur on non-lexical HEADs, which is responsible for incorrect predictions with respect to RUDs.

This restriction on the instantiation of slash features is imposed for a number of reasons. It is central to the GPSG account of parasitic gaps (about which more below) and it also provides a way of accounting for the ungrammaticality of examples like (14), as pointed out by Flickinger (1983)

14. *Which enemy did John mourn the destruction of the city by ___?

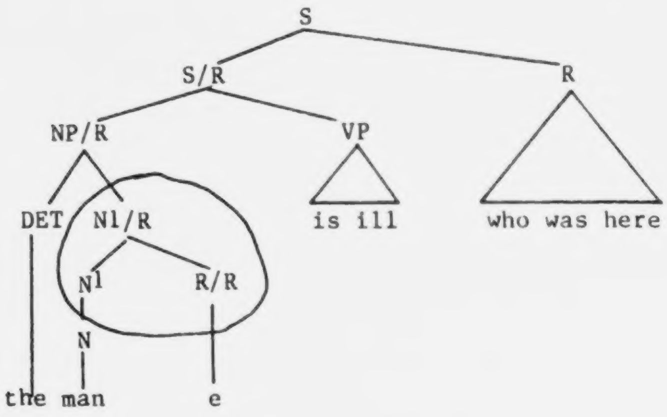
Flickinger argues that passive by-phrases in NPs are sisters of N1, not of N. Since N1 is a non-lexical head, this means that the HFC will only allow a slash feature to be instantiated on a sister PP if it is also instantiated on the N^1 itself. Since the N^1 which dominates 'city' in (14) does not contain a gap, it violates the HFC.

Once we turn our attention to RUDs, however, we find that the theory make the wrong predictions. From Flickinger's example (=(14)), we would expect the following to be ungrammatical, but it isn't, even thought it has an NP extraction site in precisely the same location as the one in (14)

15. The export of raw materials by ___ and import of finished goods to ___, third world countries, is a matter which has been receiving much attention

To return to some of the examples used earlier, it turns out that we not only find violations of the LHC, but also simultaneous violations of the HFC. (3), for example, repeated here

3.



contains a subtree (circled) which violates the HFC, since the non-lexical head, N1, does not contain [SLASH R].

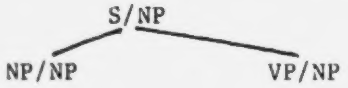
It is clear from the above discussion that GPSG can no longer claim to have a viable analysis of RNR, or RUDs generally. One possible conclusion from the above, suggested by McCloskey (1986), is that these constructions do not involve slash categories at all. Nonetheless, the problems that we have been discussing in connection with RUDs crop up in a similar fashion in the GPSG analysis of a construction which seems to present an uncontentious instance of a slash category analysis - parasitic gaps.

Parasitic Gaps

GKPS (162-7) present an analysis of parasitic gaps which they claim is 'as close to optimal as any we know of' (166) and in which the properties of parasitic gap constructions require no special stipulations, but arise as 'a consequence of the general character of our treatment of unbounded dependencies' (167).

Recall from the discussion above that instantiated slash features are constrained by the HFC to appear on non-lexical heads. Parasitic gaps arise as a function of the FFP, which permits FOOT features (including SLASH) to be instantiated on any non-lexical daughter in a tree. Thus, the combination of the HFC and FFP will admit local trees such as (16a), in which the <SLASH,NP> on the VP is required by the HFC while <SLASH,NP> on the subject NP is permitted (but not required) by the FFP, and hence the grammar will admit sentences containing such structures as (16b). (GKPS:164)

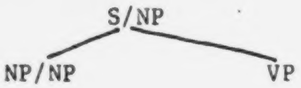
16.a.



16.b. Kim wondered which authors [S/NP [NP/NP reviewers of ___] [VP/NP always detest ___]]

Note that (17a) is not allowed by the theory. [SLASH NP] has not been instantiated on the head, resulting in a violation of the HFC, and sentences with this structure, such as (17b) are, indeed, ungrammatical.

17.a.



17.b. *Kim wondered which authors [S/NP [NP/NP reviewers of ___] [VP always detest Shakespeare]]

(16) is an example of a parasitic gap in a subject NP. It also incidentally happens to involve an extraction site which is inside a subordinate clause. As it happens this latter property is rather critical for the adequacy of GKPS's claims. If we modify the example so that the clause involved is a root clause, GKPS's analysis fails to make the correct predictions. (18) exemplifies one possibility, which is indeed consonant with GKPS's

analysis.

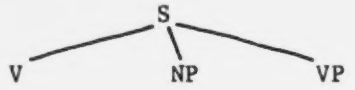
18. Which authors did [NP/NP reviewers of ___] [VP/NP always detest ___]

Unfortunately, their analysis also admits (19).

19. Which authors did [NP/NP reviewers of ___] [VP always detest Shakespeare]

The reason for the difference in predictions between (17b) and (19) is GKPS's analysis of Subject Auxiliary Inversion. The structure they assign to examples like (19) is the flat one in (20).

20.

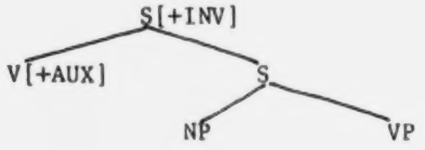


The problem here is that VP is not the head of S, instead V is.[3]

Since the head of (20) is lexical, the HFC is here irrelevant to slash feature instantiation and the only relevant feature instantiation principle is the FFP, which only requires that SLASH be instantiated on at least one non-lexical daughter.

This problem would of course disappear if one were to revert to the binary branching analysis of SAI advocated in Gazdar et al (1982), since the HFC would force the VP (the head of S) to contain any instantiated slash feature specification in

21.



But this is an option which is not open to GKPS. They point out that they reject this earlier analysis on empirical grounds:

> 'it exacerbates the problem of correctly assigning nominative case, it entails a very artificial analysis of copula constructions, and it provides no way at all of handling such British English examples as "Have you a match?"' (73.fn3)

Furthermore, they also point out (69) that the version of the SAI metarule required by the earlier treatment violates a further constraint which they impose on metarules, namely that there be a maximum of ONE category expression on the right hand side of a metarule pattern. There is also a further problem with the binary branching analysis which they don't comment on. The CAP as defined in GKPS will not enforce agreement between the subject NP and the V[+AUX] in structures like (21), as is required:[4]

22. Is he going?

23. *Are he going?

I conclude that the theory proposed by GKPS, does not offer an analysis of these constructions which is 'close to optimal', and that, in fact, these constructions raise problems which strike at the heart of some of the central principles of GKPS' version of GPSG.

FOOTNOTES

[1] In GKPS, one or more categories on the right-hand side of a rule are stipulated to be the head(s) of the rule. The Head Feature Convention (GKPS:97) requires that, if no specification of the bar level of the head is given in the rule, the instantiated value of BAR will be identical to the that of the mother. Therefore, the head in (5) will receive the instantiated feature specification <BAR, 2>, and the head in (7) and (8) <BAR, 1>. A Feature Coocurrence Restriction stipulates that SUBCAT is only defined for categories of <BAR,0>, so none of these rules satisfies the definition of a lexical rule.

[2] Although RUDs are much less constrained than LUDs with respect to possible extraction sites, there is a least one situation in which they are more restricted. LUDs are subject to the 'that-trace' effect, as (5) illustrates, but, as is well-known, leftward extraction of subject NPs is OK provided the complementiser is not present.

i. Who did you say ___ went?

RUDs on the other hand are impossible from subject position, irrespective of the presence of a complementiser.

ii. *I think (that) ___ may be going tomorrow, and know (that) ___ will certainly be there next week, that man I was telling you about.

In this respect, the LHC is actually too liberal.

[3] This is clear from the rules used to define structures like (20). They are derived by metarule from VP rules (GKPS:63-5).

i. VP --> W ==> S[+INV] --> W, NP

All rules contain stipulations as to which daughter is the head, and the application of (i) to

ii. VP[+AUX] --> H[n], VP

gives the rule

iii. S[+INV] --> H[n], VP, NP

which is ultimately responsible for (20).

4 But, since it won't handle agreement in structures like (20) either (cf Borsley, 1984), perhaps this is not a significant count against (21).

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SOME RHYTHM, RESONANCE AND QUALITY VARIATIONS IN URBAN TYNESIDE SPEECH

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Writers on English pronunciation often comment on the different vowel qualities which can be heard, in different accents, in the final syllable of words such as *city* (e.g. Jones, 1972; Ripman, 1940; Ward, 1950; Wells, 1983). Wells, for instance, writes:

> Most RP, and conservative varieties of GenAm have [ɪ] for *happY*. This quality is also found in the centre of the north of England (Manchester, Leeds) ... Rather opener qualities approaching [ɛ] are found in Nottingham and in certain varieties of RP (particularly that associated with army officers) .... Consistent final [i] is found in much of the south of England, as well as in the peripheral north (Liverpool, Newcastle, Hull, Birmingham). (1983: 165-166)

Recent work on the speech of localised urban Tynesiders has revealed some interesting and, to the best of my knowledge, previously unreported aspects of vowel variation in such final open syllables. This work indicates that the final vowel in *city*, etc. may not be as 'consistent' as Wells suggests.

Here I report on the speech of seven localised urban Tyneside speakers (three men and four women) however the observations seem to hold for other speakers on urban Tyneside (see Local, in prep). I suspect that some of the variation that I will discuss has sociolinguistic relevance for the Tyneside community but at present this is little more than a hunch. (Jones-Sargent, 1983, gives details of some aspects of sociolinguistic variation in the Tyneside community.) The tape recordings on which the present observations are based were made during the course of research for the Tyneside Linguistic Survey (Pellowe et al, 1972).

For the Tyneside speakers considered here the vowel qualities in the final syllable of words such as *city*, *Geordie*, etc. are, as Wells suggests, typically in the close front spread region. However, on careful listening there are several recurrent qualities discernible within this region. The following symbols for these qualities

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appear in my transcriptions: [ˈɪ̟ː ˈɪ̞ː ɪ̞ ɪ̞̈ ɪ̠ ɪ̞ ɪ̞ ɨ ɨ̞ ɨ̞̈ ɪ ɪ̈]. The range of variation which these symbols reflect is not random. It is explicable if we take account of three things: (1) the resonance characteristics associated with the articulatory gesture preceding the final vowel, (2) the rhythmic-quantity characteristics of the first, accented, syllable and (3) the voicing characteristics immediately following the final vowel.

(1) Closer, more peripheral qualities, e.g. [ɪ̞ ɪ̞] are observed where preceding gestures have clear resonance. For the Tyneside speakers under consideration here clear resonance is associated with alveolarity laterality and voice. and with nasalitv with alveolarity or bilabiality and voice. More retracted or central qualities, e g [ɨ ɪ ɪ̈] are observed where preceding gestures have central-to-back or back resonance. These resonance categories are associated with labiodentality with close approximation or open approximation and voice and with apicality, post-alveolarity and voice. All other preceding articulatory gestures have central resonance associated with them and are associated with the following typical, final, qualities [ɪ̞ ɪ̞̈].

Thus for one speaker (PT) for pally, any, heavy, carry, happy, hacky and coffee we find:

pʰa̱ɫɪ̞ ɛ̱ɳɪ̞

hɛ̱vɨ̞̈ kʰa̱ɹɪ

ha̱ˀpᵊ·ɪ̞̈ ha̱ˀkᵊ⁻ɪ̞̈ kʰɒfᵊɪ̞̈

(Transcriptions are no narrower than is necessary to give some idea of pronunciation, to indicate resonance characteristics where relevant, and to indicate the quality of the final vowel. Thus S̱ = 'clear' resonance; Sᵊ = central resonance; S̴ = central-to-back or 'dark' resonance. Where S = any symbol.)

(2) The rhythmic-quantity characteristics which have relevance for the quality of these final open syllable vowels are those discussed by Abercrombie (1964). Abercrombie distinguishes, for disyllabic feet, three rhythmic-quantity configurations: short-long, equal-equal, and long-short. The third of these does not concern us here but the first two do. For the seven Tyneside speakers disyllables of the kind busy, city and very have first syllables which are noticeably shorter than their final syllables. In words such as country, handy, easy and tiny

these speakers produce syllables of roughly equal length. The configuration of events which give rise to these two syllable quantity patterns are those which Abercrombie describes. Short-long quantities are found in disyllables where the first, accented, syllable has a short vowel followed by no more than one consonant. Equal-equal syllable quantities are found where the first, accented, syllable has a short vowel followed by more than one consonant, or a long vowel followed by any number of consonants. For all seven speakers words with short-long quantities have (as a set) less peripheral, less close vowels in their final unaccented open syllable. Words with equal-equal quantities have more peripheral, closer vowels in their final syllables. Thus for one speaker (PT) we find for silly, sunny, sorry, happy, touchy, coffee, (short-long), and ghastly, tiny, country, auntie, lumpy, poky, leafy (equal-equal) we find:

sɪɬi̞ sɔ̟ɲi̞ sɒ̟ɹï ha̠ʔpˀ-i̞ tʰɤ̈tˀʃi̞ kʰɒ̟fˀi̞

gæsɬi̞ tʰɛ̞ːi̞ɲi̞ kʰɤ̈ɲʔtɹi̇ a̠nʔtˀ-i̞

ɫɤ̈mʔpˀ-i̞ pʰöːˀʔkˀ-i̞ li̞ːfˀi̞

(It is interesting to note that for this speaker, and I think for many other Tyneside speakers tʃ , as in touchy, functions, for rhythmic purposes, as one consonant. Compare this with Abercrombie who remarks 'for me tʃ functions as CC and not as C'. (1964: 33)

For another speaker (IW) for smelly, worry, city, body, busy, (short-long), and Lobley, hungry, forty, eighty, Clarkies, nosey (equal-equal) we find:

smɛ̞ɬi̞ wɔ̟ɹi̇ / wɔ̟ɹï

si̞ʔtˀ-i̞ bɒ̟dˀi̞ bi̞zˀi̞

lɒ̟bɬi̇ hɔ̟ŋɡɹi̞/ hɔ̟ŋɡɹi̇

fɔ̟ːʔtˀ-i̞ i̞ːˀʔtˀ-i̞ kla̟ːʔkˀ-i̞z no̟ːˀzˀi̞

For another speaker (FTB) for canny, every, netty, kiddie, mucky (short-long), and tiny, Mary, forty, boodie, bookies (equal-equal) we find:

kʰa̠ɳi̞ ɔvɹ̝ɪ ne̞ʔt̉⁻i̞ kʰɪ̞di̞ mə̟ʔk̉⁻i̞

tʰɛ̞ɪɳi̞ me̞ːɹ̝ᵻ fɔ̟ːʔt̉⁻i̞ bu̟ːd̉i̞ bu̟ˑʔk̉⁻i̞z

(3) The quality of some of the final syllable vowels under discussion here also varies depending on where there is following silent pause or a voiceless articulatory gesture on one hand, or following voice on the other (cf. Jones, 1972: para 260ff; Wells, 1983: 165-166). For words with short-long quantity the final vowel is relatively closer if there is following voice. Thus words with short-long quantity are maximally differentiated, other things being equal, from those with equal-equal quantity, in respect of their final vowels when pre-pausal or when the following articulation is voiceless. Thus for instance (for FTB) we find:

very poor...	ve̞ɹ̝ï
very good ...	veɹ̝i̞
rake in the money	mə̣ɳi̞
such a lot of money when they went on holiday	mə̟ɳi̞
money to spend ...	məɳi̞

(for PT):

very happy	ha̠ʔp̉⁻i̞
happy living down here	ha̠ʔp̉⁻i̞
get touchy	tʰɤ̈t̉ʃi̞
touchy about ages	tʰɤ̈t̉ʃi̞
I could maybe	mɛ̱b̉i̞
maybe when both the children ...	mɛ̱β̉i̞

(for MJF):

find any	ɛ̞ɳᵻ
got any	ɛ̞ɳi̞

any time	e̝ ɳ ɪ̝
any money	e̝ ɾ̃ ɪ̝
any day	e̝ ɳ ɪ̝

(for JH):

money	mɤ̈ɳɪ̝
money now	mɤ̈ɳɪ̝

In particular pre-voice positions, where the voiced element is a hesitation noise (typically [ɛ̈ː ɛ̝ː e̝ː]), diphthongs with close and long second elements sometimes occur. Such diphthongs for these final unaccented vowels only ever occur here and when speakers can be said to be doing 'turn holding' in conversation (cf. Local, Kelly and Wells, 1983).

In summary then we find the following typical pre-pausal qualities for the final open syllable vowel under discussion (using superscript ʸ for clear resonance, ᵊ for central resonance and ʷ for central-to-back or back resonance).

Words with 'short-long' patterns:

C V $\overset{y}{\mathrm{C}}$ V	ɪ̝	e.g. silly
C V $\overset{ə}{\mathrm{C}}$ V	ɪ̝̤	city
C V $\overset{w}{\mathrm{C}}$ V	ɨ̤ ɩ ɩ̈	very

Words with 'equal-equal' patterns:

C V $\overset{y}{\mathrm{C}}$ V	ɪ̣	e.g. tiny
C V $\overset{ə}{\mathrm{C}}$ V	ɪ̝	lumpy
C V $\overset{w}{\mathrm{C}}$ V	ɨ̇	nervy

Closer and front qualities occur for 'short-long' pieces where there is following voice.

The data I have discussed briefly here has, I think, three main implications for phonological statement. First, it highlights the need to recognise, in analysis and statement, many structures and systems. Here, for instance, unless those words with 'short-long' patterns are dealt with separately from those with 'equal-equal' patterns and treated as different structures having different properties, we would have no principled way of sorting out variation of the kind I have discussed. We could not, for instance, give a coherent description of the vowel quality variation and overlap in

a̠̣nˀt̚ˉi̜   lɤ̈mˀp̚ˉi̜   li̠̜ːf̚i̠̜   tʰɛ̠ːi̠̜ɳi̜

gəsɭi̜   so̟̜ɳi̠̜   smɛ̠̣ɭi̠̜

on one hand and that in

smɛ̠̣ɭi̠̜ # sɪɭi̜ (boy)

so̟̜ɳi̠̜ # sɤ̈ɳi̜ (garden)

on the other. In the first group the overlap-variation results from the interaction of resonance categories and rhythmic patterning. In the second group the variation results from the same rhythmic patterning and resonance category ('short-long', clear) but with the presence or absence of following voice. If the corpus was extended to include other unaccented final open-syllable vowels the need for polysystemic treatment would become even more obvious. In the case of words such as latter, laughter, widow and window the rhythmic characteristics discussed above have no consequence for final vowel quality. Rather, in these cases, any quality variation which may occur seem to have to do with systems of vowel harmony focussed on the first, accented, syllable (see Local, in prep).

Second, the data here draws attention to the need to take systematic account of the resonance characteristics of all articulatory gestures (see Kelly and Local, in prep.). If such characteristics had been ignored here there would have been no way of accounting for the similarities of effect of [ɭ ɱ ɳ] on one hand and [˖ ˖] on the other and the difference of effect of both of these sets from all the other consonants standing before the final vowel.

Third, the importance of the rhythmic piece as prime-mover in the determination of vowel quality here emphasises the phonological relevance of pieces longer than the segment or syllable. The relevance of such pieces is all too often overlooked in treatments of English phonology.

The type of vowel variation that I have discussed here may not be restricted solely to urban Tyneside. I have heard quality variations which seem to go around with the same rhythmic patternings from a West Yorkshire speaker. Moreover, it is interesting to speculate that such rhythm and vowel quality variations may not be restricted to present-day English. For example, Abercrombie, while discussing the anomolousness of `a syllable which is stressed and yet short, followed by an unstressed one which is long' (1964:30), notes that Sweet was interested in the phonological relevance of this rhythmic pattern for language change. Abercrombie writes:

> Sweet considered . . . that this relation of quantity goes back a long way in English and accounts for the retention of the final *u* in OE *scipu* as compared with its loss in *hus* or *word*. (1964:30)

The present data tantalizingly suggest that the OE quantity relationships Sweet was concerned with may well have had *quality* implications and that these might have contributed to the language change and non-change.

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A METHODOLOGY FOR DESCRIBING SOCIO-LINGUISTIC VARIABILITY WITHIN MULTI-LINGUAL SETTINGS IN GENERAL, AND `INTERACTIVE' AND `REACTIVE' ETHNIC PROCESSES IN LANGUAGE IN PARTICULAR

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Abstract

After mentioning the social context where I am conducting my research (and where the ideas outlined below have yet to be put to the test), this paper briefly refers to work on the English of British schoolchildren of South Asian extraction. It then speculates on the relationship between their English and two ethnic processes - what Gumperz calls the `interactive' and the `reactive'. The way in which Network Analysis (as used by Gal and L Milroy) permits an investigation of the first of these is outlined, and then consideration is given to a means of examining the second (the `reactive'). This is Identity Structure Analysis (ISA), developed by P Weinreich, which in addition provides a systematic method for discovering what it feels like <u>inside</u> a network. The paper ends with a bolder claim for this combination of Network Analysis with ISA. Together, they give empirical and economical realisation to several important components in Le Page's sociolinguistic hypothesis and riders. Depending on the adequacy and status of this theory, the methodology described here covers parameters that are really the most fundamental to any (neighbourhood) sociolinguistic survey (whose main focus is on the language of the individual speaker). [1,2]

The social context of this study

(Since this study does not entail any large scale survey, my account of this is very brief and imprecise.) Bedford is a town of about 90,000 inhabitants and it has a large ethnic minority population. Since the war, migrants from over 50 countries have settled in the town and in 1970, according to Brown, ethnic minorities formed 20 per cent of the population, a quarter of the children in schools, and a third of all births. The largest groupings are, in order of settlement, Poles, Italians, West Indians, Indians, Pakistanis and Bangladeshis.

My study is based in just one area of the town where there is a relatively high ethnic concentration and where a

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number of languages are spoken: for example, varieties of Italian, Punjabi, Bengali, Creole and English. And its main but not exclusive interest is in second and third generation 12 to 16 year olds of Indian and Pakistani descent whose parents at least, speak a variety of Punjabi. How and why is it that if, for example, you stand in a school dinner queue listening to children behind you talking to one another in English, you can very often (though not infallibly) distinguish children of Asian extraction from children of West Indian and English extraction before you look round?[3]

Studies of Asian children's English in Britain and an unresolved issue: which type of ethnic process is involved?

Several studies of the English of Indian and Pakistani children have been produced for educational consumption, but on the whole their emphasis has been normative. The implication has been that where Asian children's English did not conform to the Standard, this was due to the failure of individuals to learn the language properly and what was needed was more and better ESL teaching. Until fairly recently, the possibility that these children's English might represent some kind of collective response to the social environment was ignored (cf Rampton 1983).

There has so far to my knowledge been only one properly sociolinguistic study of Asian children's spoken English and this was carried out by R K Agnihotri under Professor Le Page's supervision. This contains a good deal of discussion of Le Page's acts of identity theory,[4] and it gives the theory empirical exploration most fully in an analysis of code mixing.[5] When it develops a correlational analysis of the English spoken by Sikh children in Leeds, however, Le Page's hypothesis and riders recede somewhat, becoming only a theoretical backdrop at some remove from the study's main empirical thrust. The social variables that Agnihotri selects are length of residence in Leeds, place of origin, residential isolation, sex and socio-economic status. Some interesting patterns emerge: for example, Kenyan Sikhs retain Indian English features longer than Indian Sikhs; likewise boys retain them longer than girls. But the reasons for such patterns can only be speculated about - do Kenyan and male Sikhs have less contact with white children than Indian and female Sikhs? Do they have a stronger sense of identity, or what? The use of fairly macro-level variables like SES, sex etc cannot really start to answer questions such as these, and this is a pity since particularly in the discussion of ethnic culture, the distinction is repeatedly made between, on the one hand, the inheritance and maintenance of cultural forms through close ingroup networks, and on the other, the selection, development and use of cultural forms to symbolise group identity in settings of intergroup contact. (Parson's tradition vs contract (1975); Wallman's interface vs identity (1978); Barth on the morphological study of culture *vs* the structural functional study of ethnic boundaries (1969); Gumperz (1982) on old interactive *vs* new reactive ethnicities). The English of Asian children might be influenced by either of these ethnic processes. Jack Richards (1972) notes:

> 'In the case of non-standard immigrant English we are presumably dealing with ... contexts in which ... there are few informal or friendship contacts with speakers of standard English ...'

He goes on: where

> 'bilinguals interact and communicate with each other, using both languages far more frequently than they interact and communicate with members of the ... mono-lingual community, ... speakers generate their own bilingual norms of correctness which may differ from the mono-lingual norms.' (repr 1974: 67 & 69)

He also adds that this 'may also become part of the expression of ethnic pride'.

There is no doubt that these two ethnic processes can overlap and interact, and that a single cultural form can both be the product of inheritance and reflect active symbolic identification with the ingroup. But equally, they need not and indeed the theoretical and educational implications of a variety of English which predominantly reflects closed network interaction, are very different from those of a variety in which the group-symbolic function is strong: in fact the implications are so different that it seems worth trying to get an estimate of the importance of each ethnic process in the English of some of the Asian children in Bedford.[6]

The value of Network Analysis

SES, area, sex etc are then variables ill-suited to illuminating quite a basic question that one would like to ask about the language used by an ethnic minority. Indeed, it may be that groups differentiated in terms of 'macro-variables' like these are not the best units with which to explore the relation of these two ethnic processes to speech. The first task should be to explore the extent to which they affect individuals within the group, and the first comparisons should be inter-individual (cf Hudson 1980: 71-72; 166-167; also McEntegart and Le Page 1982: 107, 123). So what social variables are appropriate?

Network analysis provides one starting point. In drawing attention to the two types of ethnic process I have described, Gumperz juxtaposes to the new 'reactive' ethnicity an account of the old interactive ethnicity which he describes as being 'supported both regionally and interpersonally through reinforced social networks which joined people through clusters of occupational, neighbourhood, familial and political ties'. (1982: 5)

Network analysis is one means of assessing this, and indeed the ways in which it is used by both Gal and Milroy are complementary. Gal's work involves differentiating individuals in terms of the content or social composition of their personal networks - she describes the extent to which their contacts over a given time involve people of peasant or worker status. Milroy's work produces a more complex picture of the structure of each individual's network - people are differentiated in terms of how embedded they are within their networks. The way to combine these two approaches would be first to identify different segments of an individual's network in terms of the ethnicity (and possibly age) of their associates, and then to estimate the density and multiplexity of each. Thus one might first divide a person's network into coethnic adults, coethnic peers and then other-ethnics; next one could assess each of these 'sectors' in terms of network criteria such as size, density, multiplexity or frequency; and then the final step would be to produce a set of indirect indices which estimated how far involvement with each sector penetrated and permeated an individual's experience of life in the community as a whole. For example, using data collected in a pilot run, I devised the indices of adult kin and other-ethnic network involvement shown below:

Two Provisional Indirect Indices of Network Involvement

Adult kin network involvement

1. Living in thesame area as at least two other kin households.
   (Kin know each other -> density;
   area + kin co-memberships -> multiplexity)

2. Regular participation in leisure activities with adult kin such as trips, outings + visits
   (-> multiplexity, if multiplexity is defined in terms of types of activity shared)

3. Regular participation in religious activity with adult kin
   (active religious co-membership -> increased multiplexity)

4. ....

Other-ethnic network involvement

1. Having other-ethnic kin
   (kinship -> multiplexity)

2. Having an other-ethnic peer who is spoken to at least

every day or most days
(-> frequency)

3. Seeing them in more than one type of setting - for example at school and in the park/at home
(-> multiplexity)

4. Having a relationship with more than two people in categories (2) and (3)
(-> size)

5. Seeing the people in (4) in the same settings.
(-> density).

These indices are of course not unproblematical,[7] but they do represent a way in which the sensitivity of Milroy's study of mono-cultural groups can be adapted to a bicultural setting like Gal's. In this manner, I think, the extent of a person's involvement with two different norm-enforcing groups can be estimated.

The value of Peter Weinreich's Identity Structure Analysis

It must of course be admitted that, even in its most effective forms, network analysis leaves a lot of gaps. As Milroy herself admits, the network scores she produces do not 'reflect an individual's personal affinities and attitudes to the vernacular culture in any consistent or reliable way' (1980: 200). Neither Milroy's nor Gal's use of network analysis produces any picture of what it looks or feels like inside a network, they say nothing about group self-consciousness and they give no indication of the climate of social relations with outgroups. A comprehensive, flexible and explicit method for approaching this task is however provided, in my view, by Peter Weinreich's work on Identity Structure Analysis. Weinreich's approach is too complex to be outlined in detail here, though it is worth describing some of its essentials.

It derives in part from Personal Construct Psychology, and the method begins by exploring each individual's view of their social environment by means of a semi-structured interview. This produces a list of influential people and groupings (called entities) and a set of constructs representing the ways in which these people and groupings are perceived. Included among these 'entities' are various selves - for example 'me as I am now', 'me as I would like to be', 'me as I used to be', 'me as (others) see me', etc - and together these entities are given ratings with regard to each of these constructs. (See the Appendix for examples.) All this then forms the input to the IDEX computer programme devised by Weinreich which then produces a variety of indices, indicating among other things, how far a person sees himself as being similar to various people and groupings at present (= current identification), how far they would like to resemble various people and groupings ideally (= idealistic identification), how far they would like to be different (= contra identification), and how far they currently see themselves as similar in spite of aspiring to be different (= identification conflict). The programme becomes more complex than this and produces indices whose meaning is less simply explained, and anyhow whose relation to sociolinguistic hypotheses becomes more obscure. But it is worth stressing the value of the approach. First of all it uses each respondent's own categories for interpreting themselves and their social worlds, and secondly, in spite of this, the programme produces indices that can be compared across individuals.

Relating this back to the discussion of social networks, it becomes clear that people and groupings selected from inside and outside each person's network can serve as entities in this procedure, and then after each respondent has rated them on a variety of constructs, an index can be produced to show, e g how much they currently identify with clear groupings or with particular teachers, Anglo or West Indian kids within their networks; or with teachers, West Indians or English kids in general. Finally, individuals can be differentiated in terms of their identification with categories they have in common. The complementarity of these analyses of Network and Identity Structure are summarised schematically below:

	Which Variables Each Analysis Measures	
The Variables Given Empirical Assessment	Network Analysis	Identity Structure Analysis
Type of regular associate	✓ (Gal)	X
Familiarity of associates with one another	✓ (Milroy's density)	X
Number of ways a person is linked to their associates	✓ (Milroy's multiplexity)	X
Attitudes towards individuals	X	✓
Attitudes towards ingroups and outgroups perceived in the environment	X	✓

The relationship of this combination to language variation

There is of course little point in emphasising the complementarity of these two approaches if they simply replicate one another in terms of their linguistic implications. Below I shall argue that this is not the case, but before doing so, it is necessary to emphasise that this combination of Network and Identity Structure Analysis is not directly appropriate to any delicate analysis of stylistic variation - it is geared to a grosser, more abstract appraisal of people's productive behaviour in English, and perhaps to making rough guesses about the linguistic systems we assume to derive from and underlie interactional performance (cf Le Page 1980: 124). In order to deal *properly* with stylistic variation, fairly delicate analysis is necessary of the statuses and co-memberships actually or potentially present in the process of interaction. Network indices take within their view a large number of separate social relationships, comprising a variety of affective and role relationships which are only very crudely distinguished (Gal), if at all (Milroy). ISA tries to give a comparatively general idea of the individuals and groups with whom an individual identifies or counter-identifies, but it permits relatively little discussion of the projection of identity within interaction (cf e g Brown and Levinson 1979; Giles and Johnson 1981). So in the ensuing discussion of the linguistic implications of NA and ISA, it is necessary to bear in mind that the conception of L involved is fairly gross, focussing more on the items comprising a chunk of a person's productive repertoire, than on their use of those items in ongoing social interactions.

Having said that, it is possible to differentiate the socio-linguistic processes covered by Network and Identity Structure analyses as follows. From Milroy we may infer that network structure affects:

(a) the L items that a person is intensively exposed to and the language items that, as a result, they have the chance to adopt;[8]

(b) the extent to which a person is subject to pressure from his associates to conform to their linguistic norms.

Identity structure analysis on the other hand may start to give an idea of a person's psychological susceptibility to the speech models perceived around him, and an idea of the speakers from whom he may wish to diverge. ISA can be seen as giving a clue to what social categories are most important and how far a kind of psychological filter may have operated in picking up or screening out the linguistic data they provide.

Of course if one is going to accept Milroy's assertion that network structure is an explanatory factor in language variation, one has to recognise that she is also *covertly implicating* some of the psychological variables covered within ISA. There would be no point in talking about group conformity pressures being an important link between people's language and their network structure if the psychological susceptibility of individuals was not also a factor. The point obviously is however, that ISA makes a factor like this explicit and that it allows one to start to explore the relationship between on the one hand, a person's position in the external world - the cultural and linguistic models available to them and the pressures put on them - and on the other, the way in which this is represented and evaluated within their own minds.

Let me give some examples of how Network Analysis and ISA can be combined in practice.

Their combination could first of all inform the analysis of linguistic variables. Holding age, sex and ethnicity constant and taking a linguistic variable like dark (ɫ) it should be possible to see for example whether a retroflex realisation correlates with lack of close contact with West Indian and English people, whether close involvement with Punjabi-speaking network sectors is critical, whether the main factor seems to be strong ingroup identification, or conversely strong outgroup counter-identification. Alternatively, is the vocalic realisation connected with involvement in multi-ethnic network clusters, is it associated with idealistic identification with Anglos etc etc?

It may be that, in most cases, patterns of psychological identification correspond quite strongly with network structure - close network involvement with co-ethnics associating with strong ingroup identification and so forth. However, there may well be some cases where a person's 'structural' position and their identification diverge, and the combination of ISA and network analysis should provide a rigorous and systematic basis for examining these, together with the way in which particular language variables are involved. Of the linguistic variables, we might, for example, ask: where there is strong identification without close network involvement, do the correlated linguistic variables tend to be Labovian stereotypes, which have limited or idiosyncratic ramifications within the linguistic system? Conversely, is the systematicity of variables primarily related to strong network ties more extensive and more socially shared? (cf footnote 8; also e g Bell's discussion of 'outgroup referee design', 1984: 190) Furthermore, and more speculatively, the scope for case studies using ISA is presumably quite extensive: for example, one might ask whether individuals who indicate that their identifications have changed - whose past identification with one group have become current identifications with another - bear traces of this in their speech? and so forth.

On a broader level, what ISA and Network Analysis together provide is a clue to the extent and manner in which Gumperz's old interactive ethnic processes, and his new reactive ones account for ethnically distinct L use. I have described his 'interactive ethnicity' in relation to Network Analysis - in contrast Gumperz defines the new reactive ethnicity as depending:

> 'less upon geographic proximity and shared occupations and more upon the highlighting of key differences separating one group from another' (1982)

Language is here serving in the demarcation of ethnic boundaries and it is this that ISA can illuminate: are there groups from which people wish to be differentiated, which are they and how strong is this desire?

A stronger claim for this methodology

I would tentatively like to make a stronger claim for the approach I have outlined and argue for its relevance beyond discussion of ethnicity. I have already suggested that ISA can give an idea of what network structure looks like from the inside, and in doing so, may help explicate some of the patterns of variability that network structure can't cope with. I would also however like to refer to Le Page's hypothesis and riders (see note 4) and first suggest that the methodology just described can be effectively used to assess the main components in this theory.

In the first place, ISA is geared to a dynamic theory that looks fairly similar to Le Page's underlying ideas. Growth, for example, is seen in terms of changing patterns of identification. Weinreich quotes Erikson: "children at different stages of their development identify with those aspects of people by which they themselves are most immediately affected" (1979b: 158). The main task of adolescence is to "resynthesise all childhood identifications in some unique way and yet in concordance with the roles offered by some wider section of society" (1979b: 157; cf Le Page 1978: 2). The identifications entailed in this process are often incompatible: "in broadening" your "set of identifications, there will necessarily be an element of rejection of certain features of other people who form the wider net of those" you identify with (1979b: 160). We are clearly in Le Page's terrain, where people are seen as changing the group or groups with which they wish to be identified, and where, we are warned, "motivation is always complex" (1975: 138).

Having said that about their theoretical compatibility, ISA in fact provides a way of exploring these matters in a more empirical and economical way than Le Page's (and indeed others') approach affords. According to McEntegart and Le Page (1982), the hypothesis demands that all social and psychological factors be taken into account at once (1982: 123) and indeed data is collected on a larger number of factors such as location, age, sex, religion, political activism, wealth, self-reported ethnicity etc (1982: 118). The next stage is identifying correlations between these and speech and it is after that that the theoretically crucial operation takes place, which involves deriving hypotheses and suggestions about people's aspirations and identifications from the emerging correlations. What matters most to Le Page is not so much the 'objective' facts of an individual's relative social position, as the way in which they represent this to themselves cognitively. Without requiring a great deal of prior sociological analysis of the environment, ISA goes to the perceived reality directly, and it explores this empirically. Rather than a person's aspirations and identifications being the subject of the researcher's suggestions and inferences, the informant in ISA is afforded a chance to express these himself in some detail. Weinreich's method then proceeds to produce quantified indices which do not involve a choice 'between statistically comparable but dehumanised answers, and linguistically and socially informative conversations' (Le Page and McEntegart 1982: 115). It no longer seems true that

> 'a high level of statistical sophistication seems to militate against anything except rather superficial observations' (1982: 115)

and indices are provided that form a basis for exploring some of the fluidity, complexity and ambiguity involved in patterns of identification (e g indices of identity conflict, identity diffusion, and self-esteem).

The contribution of network analysis to the greater economy and empiricism of my approach to Le Page's theory does not seem to be quite so clear. A case for its economy would relate to Milroy's (speculative) argument that network structure is at a lower level of abstraction and is more concretely related to language than macro-variables like SES - indeed she extends this to speculation that network structure is also an intervening variable between language and age and sex (1980: 194). If one accepts this, then by addressing oneself to NS directly, one can again claim to be getting closer to the crucial mechanisms and cutting down on the levels of inference, i e being more direct. Even if one doesn't, a case can still be made that Network Analysis offers a better (though still crude) empirical metric of two factors in Le Page's hypothesis than he has used, namely (a) the access an individual has to other groups, and (b) the extent to which society provides him with feedback indicating what chance he has of success in his proposed identity (close networks presumably deter people more strongly from adopting outgroup identities than open ones - cf the norm-enforcing mechanism).[9]

If this methodology really does operationalise important factors in Le Page's theory and the theory is itself to be believed, then a stronger claim might be made, namely that the parameters covered here are really the most fundamental to any socio-linguistic survey whose main focus of interest is the language of the individual speaker.[10] This is because in contrast to SES, location, length of residence, religion etc the variables identified here by ISA (and to a lesser extent Network Analysis) are said and logically seem to be in some sense causal (McEntegart and Le Page 1982: 122). If one believes this, one may expect as a result, that the methodology I've described is able to make sense of more social variation in speech than many other approaches (e.g. Labov's, Le Page's, Agnihotri's, Milroy's use of Network Analysis on its own, ...). This methodology should provide plenty to say about socio-linguistic patterning amongst a group of co-ethnic male teenagers, before it is necessary to draw in their sisters and resort to a more conventional, less delicate socio-linguistic design.

If it does not work out as I hope, in spite of my operationalisation of its components having been satisfactory, perhaps a question arises about the status of Le Page's theory. Does it suggest empirical parameters that can be usefully handled in a neighbourhood survey or does it entail a set of socio-psychological variables that are really too delicate for quantificational analysis? Is its value exclusively conceptual/philosophical? Certainly with regard to the analysis of interaction, rather than being too delicate, the formulation of the theory may be too general.[11] There can be little doubt that Le Page's theory is important in providing a framework for much productive socio-linguistic discussion of a general nature. Whether its formulation is such that it serves as a reliable and comprehensive guide for empirical exploration remains to be seen.

FOOTNOTES

1 I am grateful to R A Hudson and Peter Skehan for comments on an earlier draft. The mistakes are mine. The work described here is being undertaken with the benefit of an ESRC studentship.

2 The frame of reference in this paper is sociolinguistic: the methodology could however be extended to studies of L2 Acquisition. One would need to take into account a variety of additional variables (such as aptitude, age, type of instruction, L distance etc etc) but even so, the ground covered here is not dissimilar to the kind of thing e g Giles and Byrne (1982) discuss.

3 I should emphasise that in referring to an ethnically distinct speech variety (here and elsewhere), I am largely talking about pronunciation. Grammatically, the speakers I am dealing with are fairly similar to their Anglo peers, and none of the items I'm interested in is very likely to interfere in writing, or to affect *formal* educational assessment.

4 Le Page's theory is as follows:

> 'Eachindividual creates systems for his verbal behaviour so that they shall resemble those of the groups with which from time to time he may wish to be identified, to the extent that
> (a) he can identify the group;
> (b) he has both opportunity and ability to analyse their behavioural systems;
> (c) his motivation is sufficiently strong to impel him to choose, and to adapt his behaviour accordingly;
> (d) he is still able to adapt his behaviour.'
> (1975)

5 One of the corollaries of Le Page's theory is that people's linguistic competence and their linguistic behaviour are a good deal more fragmentary, unsystematic and unpredictable than linguists have often assumed. What Agnihotri does, in accordance with this, is to falsify some of the rules for Indian-English code alternation formulated by Kachru and Gupta.

6 *Educational implications*:
If accented English reflected closed network interaction and was not felt to be a badge of ethnic pride, one might, if the speaker wanted to assimilate, teach pronunciation (or 'elocution'). However, one presumably wouldn't try this if it consciously carried an ethnic identification function. Whether an item reflected lack of contact with native models or ethnic group self-differentiation would influence whether one wanted to deal with that item pedagogically or not, and if so, the manner in which one approached it.

Theoretical implications:
(These are too many and too complex to go into here, though e g Tajfel, and Paulston and Paulston provide terms in which it is useful to think about this.) If Asian English appeared largely to reflect exclusively coethnic interaction, and not identification with the ingroup, one might suppose it to be relatively transitional, perhaps reflecting processes of assimilation. In this context, Asian English might be a source of linguistic insecurity, or at best the object of tacit unarticulated prestige. The identity marking function might be preserved in the Mother Tongue, with the two languages being fairly distinct

in their roles. On the other hand, if Asian English did appear to develop as an ethnic marker, the question one would then need to ask would be 'what kind of ethnic identification does it represent?' What are its goals and what perceptions of intergroup status relations does it derive from (this question might be fruitfully posed in terms of Tajfel's perceived stability and legitimacy - c f, e g Tajfel 1978)? Does it represent rejection of the dominant group, conflict, competition, aspirations for structural incorporation but cultural autonomy or what? How are each of these related to language: does the Mother Tongue still retain its identity marking function? What are the prospects for Asian English? What kind of prestige does it carry - overt prestige articulated in terms close to the dominant value system, or an overt prestige defined in oppositional terms? Can we see a process by which previously negatively defined characteristics are being revalued? - etc etc.

7 But in their defence, I would say that indeed Milroy's indirect index is not without its problems. For example, the notion of multiplexity essentially relates to the number of emically discerned major components comprising a relationship. None of Milroy's indicators properly cater for this: Milroy assumes aprioristically that different settings entail different types of co-membership but she does not verify it. As I see it, network analysis involves improvisation: in the case of Milroy at least, we can see the notion of setting being used in a manner that has neither the coherent rigour involved in the discussion of 'domain', nor the ethnographic rigour involved in the definition of 'scene'. (Fishman 1965; Hymes 1972).

8 Milroy (1980: 196), and Milroy and Milroy (1984: 38) refer to network structure affecting the linguistic items a person is exposed to, but they do not explicate precisely what they mean by exposure and in what ways network structure counts. On the face of it, any claim that network analysis can indicate the L items to which people are exposed, seems rather inadequate for two reasons. (a) We live in an electronic age. Even the people most enmeshed within a network probably have access to TVs and radios, and through these they will be exposed to a large number of language varieties. Indeed, if they recognise them, then these varieties must be said to form part of their receptive repertories (at least).

(b) There cannot be very many people whose social ties are exclusively dense and multiplex. Milroy and Milroy (1984: 44) have to admit that even the most closely involved members of the social networks they study sometimes go shopping in town etc., and thereby have plenty of chances to come across new linguistic items.

So what is the validity of the claim that network structure influences the linguistic forms a person is exposed to? In fact, there are two ways of making sense of it. The first is to say that it really relates to rather complex linguistic rules: unless you have the sustained and close contact with someone that fairly strong network ties bring, you cannot properly appreciate e g the variable constraints governing the uses of a particular variable, or maybe their precise ordering (as per Chambers and Trudgill, 1980: 153, 154, 160; more generally, c f Le Page's rider (b)).

The other way of justifying this claim that network structure influences the items a person is exposed to, is - as I've tried - to amplify this notion of 'exposure' so that it encompasses the process by which a new item passes from the receptive to the productive repertory (it is, after all, on the basis of the production that the sociolinguist usually decides whether a person 'has' the item or not). It is the passing from (vague) receptive recognition to successful productive use that network structure influences: to be able to use a new rule (depending of course to some extent on the rule involved), you need a lot of opportunities to try it out and practice it in interactional settings with plenty of positive feedback - neither TV nor shop assistants can provide this.

Of course social psychological factors will also play a role in the extent to which an L item passes into a person's productive repertoire (they will influence how far a person dares use it etc, as well as which L items are selected). There's obviously no such thing as quantity without quality of contact. But the case can still be made, that fairly extensive social contact may often be an indispensable component in the mechanics of successfully hypothesising, testing and then auto-matising (Faerch and Kasper, 1983: 53) a linguistic item. Thus, reverting to my argument in the main text and focusing on this contact/exposure issue, the relationships of Network Analysis and ISA to socio-linguistic processes can still be seen to be partially separable in principle. Though it never allows one to separate out quantity of contact per se, network analysis does entail a quantity-of-contact factor that ISA omits (Equally ISA allows a view of the psychological impact of distant people + groups which Network Analysis can't give.) Indeed, following the logic above it might turn out that empirically they each tend to correlate with different types of linguistic variable - ISA tending to be linked with Labovian stereotypes which have limited or idiosyncratic ramifications with the L system, and network analysis relating to (more covert?) variables whose systematicity is more extensive and more socially shared.

9 ISA in fact affords a way of operationalising the second of these, in such a way that one could get an idea of a person's own estimate of his chances of gaining acceptance in new identities.

10 Which is primarily interested in the question ‘"how and why do individual grammars differ?" or alternatively "how and why do individual grammars agree with each other"’ (Hudson, 1980: 189).

11 Acts of identity, for example, arguably are not conceived in a way that is sufficiently sensitive to the constraints placed on speakers in interaction: they aren't suited to coping with the pressures of different audience roles (Bell, 1984: 201), they do not give adequate attention to status relations within interaction or to people enacting role-appropriate behaviour, and thereby tend towards a barrage-of-signals view of social marking (Hewitt, 1983; Brown and Levinson, 1979). Also they are not geared to people reproducing speech associated with particular registers rather than individuals or groups (though see McEntegart and Le Page, 1982: 113; Le Page, 1980: 131). Even so the caveat that ‘motivation is always complex’ (Le Page, 1975: 138) is attached to the theory, which implies a recognition that modifications such as these may be necessary.

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APPENDIX

An example of the constructs used with one 14 year old boy.

Elicited

can't trust them - can trust them
noisy - quiet and peaceful
not friendly and kind - friendly and kind
has a hard life - doesn't have a hard life
doesn't have respect - has respect
gives you something if you ask for it - won't give you something if you ask for it
moves out of the way of trouble - gets in trouble
will tell your family if you do wrong - won't tell your family if you do wrong
ask you to their house - don't ask you to their house
jealous - not jealous
likes England more than Pakistan - likes Pakistan more than England
has freedom - don't have freedom
gets angry - keeps calm

Supplied

seems very Pakistani - doesn't seem very Pakistani
different from me - similar to me
has language problems - doesn't have language problems

His entities and a rating sheet

	Noisy	4 3 2 1 0 1 2 3 4	Quiet and peaceful
Me as I am now		- - - - 0 - - - -	01
Me as I would like to be		- - - - 0 - - - -	02
My dad and mum		- - - - 0 - - - -	03
My brothers and sisters		- - - - 0 - - - -	04
Me when I'm at school		- - - - 0 - - - -	05
Me with my family and relatives		- - - - 0 - - - -	06
Jamaican kids		- - - - 0 - - - -	07
Teachers		- - - - 0 - - - -	08
My uncles and aunts in England		- - - - 0 - - - -	09
My cousins		- - - - 0 - - - -	10
A good person		- - - - 0 - - - -	11
A bad person		- - - - 0 - - - -	12
My best friends in England (Tahir, Mohammed and them)		- - - - 0 - - - -	13
My friends in Pakistan		- - - - 0 - - - -	14
Me when I'm speaking English		- - - - 0 - - - -	15
Me when I'm speaking Punjabi		- - - - 0 - - - -	16
My English friends		- - - - 0 - - - -	17
Other English kids		- - - - 0 - - - -	18
Pakistani people		- - - - 0 - - - -	19
English people		- - - - 0 - - - -	20
Indian people		- - - - 0 - - - -	21
		- - - - 0 - - - -	22
		- - - - 0 - - - -	23
		- - - - 0 - - - -	24
		- - - - 0 - - - -	25

ELLIPSIS CONDITIONS AND THE STATUS OF THE ENGLISH COPULA*

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1. *The Problem*

Constraints on ellipsis after auxiliaries set us a little-noticed and little-discussed puzzle with an important historical dimension. In this paper I want to present a solution involving a claim about the morphological classification of the copula and its incorporation of tense-distinctions. The framework is that of Generalized Phrase Structure Grammar, but much of the discussion will be untechnical and essential aspects of the solution should be transportable to other frameworks.

The puzzle is simply stated. What underlies the distinction between the types of 1 and 2?

(1)a. If John behaves well, then Mary probably will too (sc. behave well).

b. John seems well-behaved today, and he often has in the past too (sc. seemed well-behaved).

(2)a. *If John is well-behaved, then Mary probably will too (sc. be well-behaved).

b. *John is well-behaved today, and he often has in the past too (sc. been well-behaved).

In each a nonfinite VP must be supplied in ellipsis from a tensed antecedent. With verbs which are not auxiliaries, like *behave* and *seem*, this is unproblematic. But all speakers of Standard English seem to agree that with *be* the type is impossible.[1] A common reaction to it is incomprehension and a failure to recognise that the retrieval of a *be*-phrase is in question. Note that the retrieval of the complement of *be* in such cases, as in 3, is straigntforward, but that this is not the type under discussion here.

(3)a. If John is well-behaved, then Mary probably will be too (sc. well-behaved).

b. John is well-behaved today, and he often has been in the past too (sc. well-behaved).

It is clear that the form of the antecedent in 2 is crucially involved, for in cases with an identical

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antecedent, as in 4, ellipsis of a *be*-phrase is often straightforward.

(4)a. John will be well-behaved, and Mary will too (sc. be well-behaved).

b. John has been well-behaved today, and he often has in the past too (sc. been well-behaved).

Thus the point here is that nonfinite *be well-behaved*, *been well-behaved* may not be retrieved from tensed *is well-behaved*, etc in sharp contrast to nonfinite *behave well*, *behaved well* which may be retrieved from a tensed antecedent.

Cases of ellipsis after a nonfinite antecedent pose a separate problem. If the antecedent and the form supplied are distinct in morphosyntactic category, then some speakers find retrieval impossible, though instances with identical categories are accepted.

(5)a. ?*Paula may be late this evening. She already has once this week (sc. been late).

b. ?*The children have been very good here. I wish they would at home (sc. be very good).

c. Paula may be late this evening. In fact I think she probably will (sc. be late).

d. The children have been very good here. I wish they had at home (sc. been very good).

Here *be* is again distinct from nonauxiliary verbs with which such retrieval is generally straightforward (see Warner 1985: 58f for some discussion). But some speakers I have questioned find a high proportion of instances like 5a,b acceptable, and even those who typically reject them accept some isolated cases. And they are occasionally to be found in English text. Thus this type looks like a distinct problem from that of 2 above which is never accepted by these speakers. I have discussed this type in Warner 1985 (see also Levin 1980, 1981 for further restrictions on the ellipsis of *be*), and here I would like to focus on the question posed by the impossibility of a finite antecedent.

There is an important historical dimension to this problem, for examples like 2 are found in earlier English. Instances to parallel 2a can be found throughout Middle English, and for Modern English until the end of the eighteenth century, cf Visser (1963-73: III First half: sections 1752ff). The last author to use such constructions that I know of is Jane Austen (cf Phillipps 1970). The type of 2b is less common but also appears.

(6)a. I think, added he, all the Charges attending it, and the Trouble you had, were defray'd by my Attorney: I order'd that they should [sc. be defrayed - ARW]. They were, Sir, said he; and Ten Thousand Thanks to you for this Goodness,... Samuel Richardson, 1740-1. Pamela, London: third edition 1741, vol 2 p129.

b. I wish our opinions were the same. But in time they will [sc. be the same - ARW]. Jane Austen, Emma. 1816. ed. by R.W. Chapman, Oxford: OUP 1923, p471 (cited from Phillipps 1970: 142).

The fact that closely related dialects of English differ in this respect is clearly of great importance. It is hard to believe that the striking solidity of the present-day rejection of the construction and the consequent sharpness of the contrast with earlier English is not based in some systematic grammatical facts. Grammatical prescription seems unlikely to be an adequate source for this contrast, even if the construction was ever the specific focus of prescription, and I know no evidence that it was. It is not mentioned in Leonard's survey (1962 cf especially VI.4). We have then an opportunity to uncover the parameters underlying this dialect difference. And to convince, any account of the absence of 2 in today's English must be supported by a plausible account of its loss.

Before turning to a particular proposal, I will make two more general points. The first is that the simple fact that the relationship between tensed is, etc and be is suppletive cannot be what accounts for the difference between 1 and 2. For one thing the relationship was suppletive in earlier English too when the type was permitted. For another, some speakers find ellipsis involving retrieval from suppletive instances natural, though reactions are rather variable. Thus Sag 1977 cited 7 as grammatical without comment despite the retrieval of go from suppletive went.

(7) Although John went to the store, Betsy didn't (sc. go to the store). Sag 1977: 8, (1.2.9a.)

It would clearly not be adequate to add a simple constraint against suppletion to the conditions required for instances with nonfinite antecedents as an account of the finite cases.

The second general point is that perfect have in ellipsis after a finite antecedent is not simply parallel to be with finite antecedent. Although examples are often rejected, there is not the sense of total impossibility found in 2, and instances where appropriate adverbials aid retrieval are sometimes relatively acceptable.

(8)a. ?*John has probably kissed his grandmother good night, but Paul won't yet (sc. have kissed his grandmother good night).

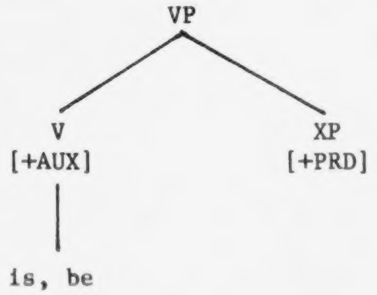
b. ?*Have you seen one yet? You should by now if they're really there (sc. have seen one).

As a group such instances are better classed with ellipsis of *be* after a nonfinite antecedent.

2. *Assumptions of Analysis*

The framework of analysis is that of Generalized Phrase Structure Grammar (GPSG) as presented in Gazdar, Klein, Pullum and Sag 1985 (GKPS). In what follows I will take for granted that predicative, `progressive' and `passive' *be* are to be identified as the same item, that it belongs to the word class `verb' and subclass `auxiliary', and that both finite and nonfinite instances occur in a structure like 9 where the predicate will carry further specification.

(9)



These positions are adequately defended in Gazdar, Pullum and Sag 1982, Warner 1985. Ellipsis of the predicate here, though often called `Verb Phrase Deletion/Ellipsis' belongs to a class involving the complement of an auxiliary. `Post-Auxiliary Ellipsis' is a better term. I will assume that such ellipses are the product of a metarule which specifies an empty complement whose meaning must be retrieved from the preceding linguistic context. In Warner 1985: 55, which generalises the treatment of Gazdar, Pullum and Sag 1982, the empty complement is XP[+NUL] where XP[+NUL] -> e. In the system of GKPS the complement will be XP[+NULL]/XP.[2]

Notice that though at first sight it might seem that the contrast between tensed and nonfinite *be* here points to a treatment in which *is* + predicate does not form a constituent, as in the popular analysis where there is a finite auxiliary position immediately dominated by S (cf Akmajian, Steele and Wasow 1979, and references), the fact that *be* and *have* differ in behaviour is against such a simple view.

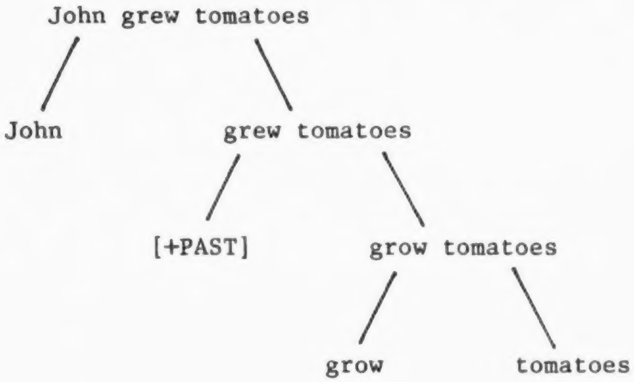
3. A Solution

I want to suggest that the tensed forms *am*, *are*, *is*, *was*, *were* (henceforth 'the *am*-forms') should be analysed in GPSG with tense incorporated as part of their basic meaning, stated in the lexicon. They differ from the majority of verbs for which the semantic interpretation of tense takes place at VP-level. This provides a straightforward account of the failure of ellipsis with the *am*-forms noted above, since with them an untensed VP-meaning is not available for retrieval.[3] This position is plausible from a morphological point of view since the *am*-forms entirely lack regular verbal inflection, a characteristic which they share with the majority of the class of modals. Moreover the position is historically plausible since the loss of the ellipsis type discussed above is coeval with the loss of characteristics shared by modals, the *am*-forms and nonauxiliary verbs. In what follows I will deal in turn with each of these topics.

3.1 A Semantic Analysis

Carlson 1983 points out that it is often the case that grammatical formatives appear, or grammatical oppositions are made overt, at a lower position in constituent structure than is appropriate for their semantic interpretation. Thus the morphological contrast of English tense appears within a verb, but the semantic scope of tense is at least VP (and perhaps also S, cf Janssen 1983). GKPS cope with this general phenomenon by assigning a translation to particular feature values (terming the features 'semantical') and ensuring that interpretation is operative at the feature's highest point of occurrence in a tree in cases where a feature percolates across different levels of structure (p223ff). For English tense they argue from coordination facts that this should be at VP level, and they assign their feature-value pair [+PAST] the interpretation PAST′ and the model theoretic type <VP,VP>. Note that GKPS use syntactic category labels to denote the corresponding model theoretic type and I will also use this convention. This means that a PTQ-type semantic analysis tree for *John grew tomatoes* is as in 10.

(10)



In GKPS semantic interpretation applies to syntactic trees, not to rules as in earlier formulations of GPSG. Each node in a syntactic tree is paired with a translation in an 'interpreted tree' by the Semantic Interpretation Schema developed in their Chapter 10. In this schema, the semantics of [+PAST] is integrated as part of the complex process of assigning a translation to the node unmarked for [PAST] which immediately dominates VP[+PAST], say S. Thus the syntactic subtree dominated by VP[+PAST] is assigned an untensed translation in the interpreted tree: it corresponds to *grow tomatoes* in 10. And the stage of semantic analysis which corresponds to the tensed *grew tomatoes* is assigned to no syntactic node in this interpreted tree; it is merely a step in the composition of the translations of the constituents of S (GKPS chapter 10, especially pp225-6).

Now, Sag and Hankamer 1984 argue that what is relevant to instances of ellipsis which, like Post-Auxiliary Ellipsis, require a linguistic antecedent is a notion of identity at a level of Logical Form which is 'highly determined by surface syntactic structure' (p329). In particular, syntactic VP corresponds to a unit in it. A natural suggestion within GPSG is that a necessary (but not sufficient) condition on retrieval in Post-Auxiliary Ellipsis is as follows:

(i) What is retrieved corresponds to a semantic expression attached to a node in an interpreted tree of the preceding discourse (where 'corresponds to' covers the relaxations of simple identity developed in Sag 1977, Sag and Hankamer 1984).

Given this, the semantics of the untensed *grow tomatoes* will be straightforwardly retrievable from the VP *grew tomatoes*. Consider now those other morphosyntactic features which are semantically interpreted. An obvious candidate is the progressive participle: [VFORM PRP] (see Warner 1985 for relevant arguments). Another likely possibility is the past participle: [VFORM PSP]. The fact that the interpretation of semantical features on VP takes place after composition of the semantics of the constituents of VP has two consequences. It means that it is only the semantics of the simple VP (*grow tomatoes*) which is retrievable from the participles (*growing tomatoes*, *grown tomatoes*) as from the tense-marked form. It also means that it is the semantics of the simple VP which is to be supplied in ellipsis, for the semantics of the appropriate morphosyntactic category will be supplied from the feature marking on the complement XP[+NULL]. Thus we correctly predict that examples like the following (cited from Pullum and Wilson 1977: 766(47)) are all grammatical.[4]

(11)a. I haven't done it yet but I will (sc. do it).

b. Harry will probably tell Sarah - in fact he probably already has (sc. told Sarah).

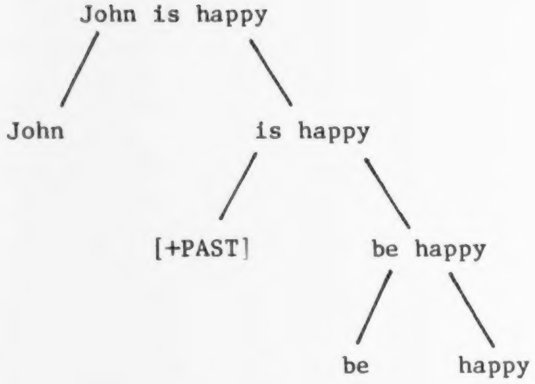
c. Max is selling hot dogs for a living, and soon all of us will have to (sc. sell hot dogs for a living).

d. I'm hoping that not all of my gerbils will die, but the weak ones already are (sc. dying).

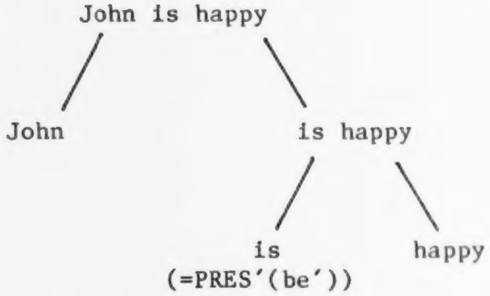
Moreover, the semantic appropriacy of what is retrieved to the site of ellipsis is automatically guaranteed for nonauxiliary verbs by Condition (i) and the operation of the Semantic Interpretation Schema, and there is no need to make a special statement to ensure this. Thus this points towards a simple and elegant account of conditions on Post-Auxiliary Ellipsis.[5] Notice, however, that a weaker version of Condition (i), requiring what is retrieved merely to be an expression of a preceding analysis tree, is sufficient for the discussion of this paper, whose points will stand if it is ultimately preferred to the stronger condition given above.

Given this, my proposal for *be* is very simple. I suggest firstly that we need to state a semantics for the present tense so that we have an operator PRES′ beside PAST′ as assumed, for example, by Dowty 1982, Cooper 1985. It will correspond to the syntactic feature-value pair [-PAST]. Secondly, I suggest that the *am*-forms are not interpreted as tensed forms of *be* with an analysis tree 12 parallel to 10 but appear in the lexicon with tense as part of their meaning. (Indeed if nonfinite *be* is to be analysed as an identity function the meaning of the *am*-forms will simply be that of tense.) The appropriate analysis tree is given in 13.

(12)



(13)



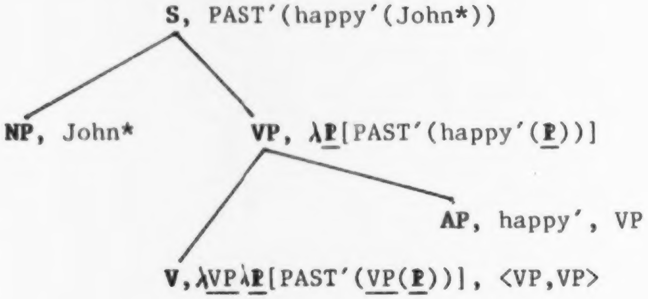
On the assumption that nonfinite be is an identity function the lexicon will supply the semantic statements of 14, and the am-forms will be entered without specification for the syntactic feature [PAST].^{6}

(14) am, is, are λVPλ𝐏[PRES′(VP(𝐏))], <VP,VP>
was, were λVPλ𝐏[PAST′(VP(𝐏))], <VP,VP>
be, been, being λVP(VP), <VP,VP>

Here 𝐏 is a variable over Den(NP), VP is a variable over Den(VP) as defined in GKPS Chapter 9.

A schematic interpreted tree (including some type information) is given in 15.

(15) John was happy.



In this tree the interpretation associated with the VP-node is tensed. Therefore under Condition (i) above it cannot supply the untensed expression required in Post-Auxiliary Ellipsis.

This leaves the problem of the semantics of the complement of *be*. Why cannot *well-behaved'* simply be retrieved from AP to supply the semantics of VP? This is impossible for predicative AP, AP[+PRD], not only in the structures considered here, but also more generally.

(16)a. What, Mary well-behaved! - Yes she was.

b. What, Mary well-behaved! - Well, she ought to *(be).

(17)a. John came early, well-behaved as ever - Yes, he always is.

b. John came early, well-behaved as ever - Yes, he feels he must *(be). [* = sc. be well-behaved]

The distinction between AP and VP here is not one of semantic type if AP[+PRD] has the type of VP as argued in GKPS: 192f. But there are two other possibilities. The first is that *be* is not an identity function, so that the semantics of *well-behaved* and *be well-behaved* is distinct, cf the analyses of *be* given in Montague 1973, Dowty, Wall and Peters 1981. The second is that a condition of syntactic identity should be added to that for retrieval given above:

(ii) What is retrieved belongs to a syntactic category which is not distinct from that required at the site of ellipsis.

This condition is suggested in Warner 1985 to cope with the ungrammaticality of ellipsis involving different morphosyntactic forms of *be* as in 5a,b above (since the categories involved are interpreted as syntactically distinct for auxiliaries though not for other verbs). It is also required to prevent retrieval of the translation of *be* + predicate (VP[+AUX]) after *do*, whose complement is VP[-AUX]. So there is some more general support for such a condition. Either of these possibilities would suffice to rule out the inappropriate retrieval of AP *well-behaved'* to supply VP in 2, 16 and 17.

Finally, what about examples like 8 (repeated here for convenience) which retrieve nonfinite *have* from antecedent tensed *have*?

(8)a. ?*John has probably kissed his grandmother good night, but Paul won't yet (sc. have kissed his grandmother good night).

b. ?*Have you seen one yet? You should by now if they're really there (sc. have seen one).

As pointed out above, these differ from the parallel cases with antecedent am-forms in being sometimes relatively acceptable. But if the translation of untensed have-phrases may sometimes be retrieved, this suggests that the semantic contribution of [+PAST] to the perfect may be integrated at VP-level as with other verbs. Then, if we assume that the type of inverted have is <VP,VP>, PTQ-style analysis trees for 8a and b will contain an expression corresponding to untensed have-phrases. In 8a it will be paired with a syntactically distinct node from that required at the site of ellipsis, in 8b with no node in the syntactic tree. The fact that retrieval is not always totally unacceptable may show the relevance of the weaker version of Condition (i) referred to above. Given that 8a, b are to be analysed as ungrammatical, we require the stronger version and Condition (ii). There is some variation in judgements, and it is possible that the weaker condition is general and that there is interpersonal variation in the application of the stronger conditions. But the general point is rather that the account of the am-forms developed here does not necessarily carry over to have any more than to the be-forms, so that it is consistent with the differences observed between these categories.

Thus within this framework it is possible to specify the meaning of the am-forms in such a way as to account for their idiosyncratic behaviour as antecedents to ellipsis. I will now try to provide some morphological and historical justification for such an analysis of the am-forms.

3.2 A Morphological Distinction

Considered as a verbal paradigm the am-forms (am, are, is, was, were) show two peculiarities. Firstly, they show no regular verbal inflection. It seems clearly better to regard the form is as invariable than as containing third singular -s. Secondly, the forms are not simply suppletive to other verbal paradigms, since they carry a different and fuller set of distinctions. It might make better sense to view them as falling outside the morphological framework of verbal distinctions, rather than suppletive variants within such a framework.

This suggestion is supported by the status of modals, for they also lack regular verbal morphology. They do not carry the third singular present -s. Their 'past tense' forms only refer to the past in a minority of uses, and they are no longer formed by rule from the corresponding 'presents'. It is striking that modal need lacks the regular nonmodal preterite needed in Standard English, that many speakers also lack the modal preterite dared, and that uninflected *dare*, *daren't* and *needn't* are pressed into service as past tenses (Huddleston 1980, Quirk et al 1985: 138-9, Visser 1963-73 III First half: sections 1348, 1363). Thus with the possible exception of the marginal *used* and the restricted *dared* (a form supported by *dare*'s tendency to blur the modal/nonmodal contrast) modals might reasonably be analysed as a distinct class of verbs at the level of morphology: one which entirely lacks verb morphology, though able to carry a contrast in tense.

But now the *am*-forms may clearly be assigned to this morphological class. In a historical perspective this is unsurprising, for the *am*-forms have long shared morphological characteristics with modals or the broader group of preterite-present verbs which contained the ancestors of most of our present-day modals. Old English *þu eart* 'thou art', *hie sindon*, *hie earon* 'they are' showed preterite-present formations (cf *þu scealt* 'thou shalt', *hie sculon* 'they shall'). Then the sixteenth century saw the establishment of the alternations *thou art~they are*, *thou wert~they were* in Standard English, replacing earlier *thou art~they be(n)*, *thou were~they were*. This again parallels (and may be partly modelled on) the interrelationship *thou shalt~they shall* found in modals (see OED Be, *v.*)

For these reasons it seems to me rather plausible to take the *am*-forms as belonging with modals to a class of verbs which lacks verb morphology (though not auxiliary morphology if *-n't* is inflectional, cf Zwicky and Pullum 1983). The *be*-forms (*be*, *been*, *being*) and forms of the auxiliaries *have* and *do* do not however belong to this class. Their inflected forms are open to analysis as both morphologically and semantically transparent, if partly irregular.

This analysis does not of course bear directly on the way tense is represented in the *am*-forms without further assumptions. But if *am*-forms are morphologically distinct from the major class of verbs, it is clearly reasonable to suggest that they may lack a further verbal characteristic.

3.3 *History*

3.3.1 Until the early nineteenth century there occur examples in which an antecedent *am*-form permits retrieval of a nonfinite VP, as in 6b above, repeated here for the reader's convenience.

(6)b. I wish our opinions were the same. But in time they will [sc. be the same - ARW]. Jane Austen, Emma. 1816. ed. by R.W. Chapman, Oxford: OUP 1923. p471 (cited from Phillipps 1970: 142).

If the account given above is appropriate, then the *am*-forms presumably carried a feature [PAST] interpreted at VP-level like other verbs until this period. Then, or perhaps at earlier periods for some speakers or styles, they were reanalysed becoming the modern holistic combinations with tense.

This development is open to a reasonable interpretation within the present framework. Two changes are particularly relevant: the increasingly distinct constructional properties of auxiliaries and nonauxiliary verbs as *do* takes on its modern distribution, and the loss of inflections in the *am*-forms (and in modals) dependent on the loss of *thou*.

(i) Loss of shared constructions. In Early Modern English auxiliary *do* and nonauxiliary verbs may both appear in inversion or before *not* where auxiliary *do* is now obligatory.

(18)a. From whence came you, and whither go you?

b. What answer did you make the villain?

(19)a. I flatter not myself with any manner of hopes.

b. He is the only man in the three Nations who does not know it.

(Examples from Dryden's prose, from Söderlind 1951-8.)

With individual lexical items full verb inversion can occur throughout the eighteenth century. But the failure of '*do*-support' is infrequent in some texts even from the first half of the century, and seems largely to be restricted to a handful of recurrent items, such as *mean*, *say*, and *think*. Loss of postverbal *not* is rather later. Examples are not uncommon throughout the eighteenth century, though again, from the second half of the century it seems to be a few recurrent items which are mainly involved.

The loss of these constructions with full verbs clearly removes important properties shared by auxiliary and nonauxiliary verbs. A grammar of English written in 1700 would have to identify tensed verbs as the locus of negation and inversion. By 1850 auxiliaries alone were involved. This loss of shared properties must clearly increase the possibility that the *am*-forms would be reanalysed as lacking a further verbal characteristic. But the fact that these forms are unlike the *be*-forms or *have* in behaviour implies that something further is involved. I take this further crucial factor to be (ii).

(ii) Inflectional loss dependent on the loss of *thou*. Before the loss of *thou*, the *am*-forms retained inflection, cf the form *wast* and the interrelationship (shared with *shall* and *will*): *art*~*are*, *wert*~*were*, *shalt*~*shall*, *wilt*~*will*.

Despite its narrow scope the rule here is simply stated: auxiliary plural stems ending in a liquid add *-t* for the *thou*-form (remember that postvocalic [r] is only lost in Standard English from the late eighteenth century). Thus before the loss of *thou* the *am*-forms retained the major verbal characteristic of agreement inflection, and were presumably analysed as a separate, largely suppletive paradigm. But this motivation for assignment to the same morphological class as nonauxiliary verbs disappears with the loss of *thou*. And this loss in standard colloquial English, used between intimates, to children, to servants and inferiors, or to show contempt, seems to belong to the second half of the eighteenth century. It is common in appropriate circumstances in Richardson's *Pamela* of 1740-1, and it appears in plays later in the century, cf Bock 1938. But nineteenth century occurrences are very infrequent, and belong to dialect or the language of prayer or heightened discourse. Thus this loss occurs at a period appropriate for the required reanalysis of the *am*-forms.

Modals, moreover, undergo a parallel loss. They had carried the normal agreement inflection for *thou* in the preterite and some present forms, cf the examples of 20.

(20) may(e)st, might(e)st, would(e)st; speak(e)st, spok(e)st.

Loss of *thou* meant loss of agreement inflection, and the development of a morphological subclass to which the *am*-forms could also be assigned. Moreover the 'preterites' of modals already had a very high degree of independence from the 'present' forms, and the interrelationship had been morphologically irregular since the sixteenth century at the latest. Thus modals were open to interpretation as a group lacking all regular verbal morphology, and perhaps as a group with 'lexical' tense (though this could also have been an earlier development). Thus modals changed morphological status at this period, whatever the precise details, in a way that supported reanalysis of the *am*-forms.

This interpretation involves multiple causation. The widening word-class gap between auxiliaries and nonauxiliaries, and the factors which led to this developing opposition clearly underlie it. But it highlights the loss of processes of verbal inflection as providing the specific occasion for change. This seems appropriate both in terms of date since the changes seem to follow one another closely (though there are obvious uncertainties in such an assessment) and because it ties in with the distributional distinction between the *am*-forms and *have* or nontensed forms of *be*. I have discussed this so far in general, word-class terms, but it may be that we need to characterize a special relationship between the loss of inflection and the loss of the type of 2. Within a Government and Binding framework one might try to relate these properties directly.[7] But there are other possible types of interconnection. It would surely be reasonable to suggest that the 'semantical' status of the tense feature here depends on transparency of tense-formation in the general case (not in the particular, because of suppletion). A principle like 21 would motivate the *am*-forms' loss of the semantical feature if loss of regular agreement inflection removed these forms from the morphological category of inflected verb.

(21) Tense features may be semantical in a category which has regular processes of tense formation.

Moreover, if the reader accepts, as suggested above, that today's *am*-forms do not carry inflection and are not suppletive to inflectional categories of the verb, then Greenberg's Universal 30, which refers to inflectional categories, is clearly highly suggestive:

(22) 'If the verb has categories of person-number or if it has categories of gender, it always has tense-mode categories' (Greenberg 1963: 93).

Perhaps, then, English maintained the analysis of [PAST] as a semantical feature with the *am*-forms only until these became distinguished by lack of agreement inflection. Thus it seems very probable that there is some more systematic interconnection involved here. But its nature requires further research.

3.3.2 There may be a further factor involved in the loss of the ellipsis type of 2. Although on the face of it the *am*-forms in Modern English have always made one more distinction than nonauxiliary verbs in the indicative paradigm, it is possible that we should interpret the loss of *thou* as leading to the hypercharacterization of this distinction. For the the first time the *am*-forms would have become hypercharacterized as a verbal paradigm, and hence less readily taken as suppletive to verbal inflections. Suppose that the person-distinctions of Present-day English involve the features of 23a and are as in b.

(23)a. [PTC] 'participant': [+PTC] = 1, 2; [-PTC] = 3.
[EGO]: [+EGO] = 1; [-EGO] = 2, 3.
[PL]: 'plural'.

b. 1st singular [+PTC, +EGO, -PL]
2nd singular [+PTC, -EGO, -PL]
3rd singular [-PTC, -EGO, -PL]
1st plural [+PTC, +EGO, +PL]
2nd plural [+PTC, -EGO, +PL]
3rd plural [-PTC, -EGO, +PL]

These features are straightforwardly related to those suggested by Sag, Gazdar, Wasow and Weisler 1985. The lexicon will then specify the following agreement categories for the present tense of *sing*, where AGR is the agreement feature of GKPS which takes instantiations of NP as its values:

(24)	*sing*	[AGR NP[+PTC, -PL]]	1, 2 sing
	sings	[AGR NP[-PTC, -PL]]	3 sing
	sing	[AGR NP[+PL]]	1, 2, 3 plural

It is not necessary to specify forms more completely granted the lexical insertion convention of GKPS: 34 according to which the syntactic node dominating a lexical item may be more fully specified than that item. And it is not possible to remove the homonymy between the forms *sing* without an equivalent disjunction unless the theoretical apparatus is extended to include negative conditions on feature value pairs as part of a lexical entry. But the identity of these forms can be stated within the morphology by redundancy rule, or as the consequence of an `elsewhere' condition. See Warner 1986 for some discussion of the feature system proposed here and of these further points.

The system for Early Modern English must represent in addition the distinction between *thou* and singular *you*. This encodes a distinction in the relationship between participants which is grammaticized only here. A rather natural way of representing this in GPSG is to introduce a feature PROFORM with two values {thou, you}.[8] The lexicon will then specify the agreement categories of 25, again leaving statements of identity to the morphological component.

(25)	*sing*	[AGR NP[+EGO, -PL]]	1 sing
	sing(e)st	[AGR NP[PROFORM thou]]	2 sing *thou*
	sing	[AGR NP[PROFORM you]]	2 sing *you*
	sings	[AGR NP[-PTC, -PL]]	3 sing
	sing	[AGR NP[+PL]]	1, 2, 3 plural

But given this, the loss of *thou* leads to a simplification of the agreement paradigm of English, from 25 to 24. This has an important consequence for *be*. The earlier verbal paradigm provides a category for each of the *am*-forms.

(26)	I am	was
	thou art	wast, wert
	you(sg) are	was, were
	he is	was
	they (etc) are	were

The later paradigm does not. The formal distinction between *am*, *was* and second singular *are*, *were*, has no place in it. Thus the *am*-forms can no longer be simply interpreted as suppletive members of a verbal paradigm.[9]

In this discussion I have made a series of assumptions about person and number distinctions and the representation of paradigms in the lexicon. They seem to me to be reasonable assumptions, but clearly I can do no more than suggest that the am-forms become hypercharacterized as a verbal paradigm when thou was lost and that this provided further motivation for their loss of verbal behaviour in ellipsis at this period.

4. Conclusion

I conclude that there is synchronic and diachronic justification for a GPSG analysis of the tensed forms of the English copula as distinct from nonauxiliary verbs in two respects: the semantics of their combination with tense is given lexically and they lack the morphology of verbal inflection. The first property underlies the idiosyncratic failure of these forms to occur as antecedents to Post-Auxiliary Ellipsis. This arises with their loss of verbal properties, in particular with the development of a class containing modals and am-forms which lacks verbal inflection. Particular advantages of the general analysis given here are that it is consistent with the distinct behaviour of auxiliary have and that it deals naturally with the loss of the ellipsis construction with tensed antecedent which is found in earlier English.

It would be possible to retreat to a vaguer and less satisfying account in which a simple (though minor) difference of category was taken to underlie the distinction discussed here. But the more explicit account is clearly preferable.

FOOTNOTES

* Thanks to Gerald Gazdar, Steve Harlow and David Reibel for their comments. But any mistakes are my own.

[1] The type is possible with will in the second clause in the English of speakers bilingual in English and Welsh. But this is readily interpreted as a calque on Welsh structures. See Warner 1985 note 31.

[2] Here [+NULL] will prevent further expansion, and the slash category will not percolate up the tree since its presence in the rule is stipulated by metarule. Note that Napoli 1985 analyses the auxiliary in Post-Auxiliary Ellipsis constructions as a propredicate. But her arguments for this position are mainly directed against a deletion analysis and are not convincing against the type of account offered here.

3 Oehrle, in Steele et al 1981: 259 n 18, apparently suggests that untensed representations may not be retrieved from *am*-form + predicate, so that retrieval fails. But *he* offers neither analysis nor justification.

4 See Warner 1985: 58f for discussion of variability in such data.

5 A syntactic condition and a Feature Cooccurrence Restriction (or restrictions) will also be required, see below. Condition (i) deals appropriately with these examples:

(a) *John was laughing, so Mary's being [sc. laughing] does not surprise me. (ok: so the suggestion that Mary was [sc. laughing]...)

(b) *Men are to dress formally, so I suppose women must be [sc. to dress formally] as well.

In dialects where *being* permits ellipsis, the translation of progressive *laughing* may not be retrieved in (a), only that of nonprogressive *laugh*. But no progressive interpretation is available after *being* which is not subcategorized for such complements (see Warner 1985). We may, however, need to ensure adequate specification of the [+NULL] complement by Feature Coccurrence Restriction to prevent the retrieval of simple *laugh*. In (b) the feature corresponding to *to* must be semantical on a node marked [+PRD]. Hence the translation of *to arrive* is not retrievable, only that of *arrive*. But this cannot be supplied in context. Notice, however, that given the account of the *am*-forms developed below, Condition (i) does not account for the failure of (c) where a tensed VP is retrieved:

(c) *If John was miserable, then Mary must [sc. have been miserable].

I suggest, however, that the answer here lies in a more sophisticated semantics which distinguishes finite and nonfinite forms. In particular, the model theoretic type of finite and nonfinite phrases should perhaps be distinguished, cf Schmerling's 1983 treatment of clause types, or Bach's 1980 'minimal revision' of PTQ.

6 We will, however, need to prevent the occurrence of *am*-forms under V and VP marked for [PAST], since lexical items may be inserted under more fully specified nodes (GKPS: 34). Avoiding the negative lexical condition ~[PAST], this can be done by the following restriction on feature cooccurrence:

FCR SUBCAT[9] $\supset$ ~[PAST] (where V[9] is *be*).

But I suspect that lacking a value for [PAST] is a more general auxiliary property and that a more general default would be appropriate.

7 One might seek to align the final loss of agreement inflection in the *am*-forms with loss of coindexing between these forms in INFL and their trace in VP, assuming that VP represents a retrievable unit of Logical Form. This would give a more direct relationship between loss of inflection and loss of ellipsis than the present account. The assumptions required would however involve a radical revision of the account in Roberts 1985, and it is not clear to me that they can be sustained.

8 I assume that PROFORM is a freely instantiated HEAD feature subject to the following FCR:

FCR [PROFORM] ⊃ [+PTC, -EGO, -PL]

This gives correct results for coordination and the occurrence of reflexives. Alternatively a feature based on the semantic opposition between *thou* and *you* might be adopted. Other pronouns would simply neutralize the distinction. But the solution above is better paralleled within GPSG.

9 I do not want to deny the general possibility of hypercharacterized paradigms, cf *aller*, *avoir*, *être* in spoken French, which distinguish between 1 sing and 2, 3 sing in the present indicative unlike other verbs.

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REVIEWS

Helen Kwok, Sentence Particles in Cantonese, Centre of Asian Studies Occasional Papers and Monographs, University of Hong Kong. 1984. 127pp HK$45.00.

After reading Helen Kwok's book on sentence particles in Cantonese, the thought occurred to me that in this area of Chinese grammar a contrast of Cantonese and Mandarin "dialects" shows an interesting divergence: not only does Cantonese speech use a far greater variety of particles than Mandarin, but the particles occur in Cantonese speech with much higher frequency. By focusing our attention on the use of particles in natural conversation, Helen Kwok deftly demonstrates for us how these sentence particles form a syntactic category which functions as an integral part of the Cantonese grammatical system. Based on her analysis of two hours of tape-recorded telephone conversations of eight native-speakers of Cantonese, she has been able to identify 28 monosyllabic and two disyllabic basic particles and 74 combinations of particles (or clusters) and associate particular meanings with them (in contrast, Li and Thompson's Mandarin Chinese, A Functional Reference Grammar lists six sentence-final particles). Drawing generously from her corpus of utterances to provide appropriate examples, she vividly illustrates how particular sentence particles are distributed in all three main types of Cantonese sentences - declarative, interrogative, and imperative. The particles fall into two broad categories - "non-status changing" and "status changing". By this it is meant that the presence of a particle may or may not change the grammatical function of the utterance, e g, particles added to declarative, interrogative, or imperative sentences do not change their original functions. However, some particles, for example "me", when added to a declarative statement will change it into a question, "Ngo hoey." 'I'm going.' - "Ngo hoey me?" 'I'm going?'.

The analysis of sentence particles helps point up syntactic differences existing between Cantonese and Mandarin: for example, the particle "ma" cannot occur at the end of a question which contains the negative morpheme "m", so that the question *"Nei m hoey ma?" 'Aren't you going?' is ungrammatical in Cantonese; however, this same question in Mandarin said as "Ni bu qu ma?" is a grammatically acceptable utterance.

While Kwok acknowledges that particles interact with intonation, she also recognizes that they constitute two different systems. Moreover, because she can get a better handle on the particles, she rightly restricts her discussion to them and says little about intonation in Cantonese. Nevertheless, just for this reason, her book should be of great interest to both the Sinologist and non-Sinologist concerned with speech act theory: Western

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linguists are quite familiar with languages such as English which must rely upon intonation and word order to transform the function of an utterance. Kwok's book now provides an opportunity for linguists to see how a typologically-distinct language such as Cantonese accomplishes this same purpose with a finite set of bound-morphemes appended to the ends of utterances.

In addition to examining the meanings of particles and their distribution in various types of sentences, Kwok also looks at the problem of how to represent the Cantonese sentence particles in written Cantonese. A very interesting characteristic of the Hong Kong Cantonese speech community has been the development of Cantonese writing, the use of special Cantonese characters (as well as letters of the English alphabet!) in conjunction with standard Chinese characters to record in written form spoken Cantonese. The choice of which character to use to represent which particle concerns those Cantonese-speakers who write in their native dialect, because at the moment writers are not entirely consistent. Kwok suggests a one-to-one system in which one character is associated with one particle.

Kwok concludes her book by pointing to two potentially interesting areas for future study, namely, the sociolinguistic and psycholinguistic investigations of the sentence particles. For example, how do such factors as the speaker's age, sex, educational level, the speech situation, formal and informal speech styles, etc. influence the use of particles among speakers? In the area of child language acquisition, how does the child acquire the particles? If we hypothesize that it is done in an ordered sequence just as he or she acquires the phonemes of Cantonese, we then must ask, which particles are learned first and which later?

Finally, although Kwok does not recommend her book as such, Sentence Particles in Cantonese can serve as a very useful tool to both the foreign student of Cantonese and even the native-speaker who want to learn more about the meanings of the particles and how they are used in natural speech.

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Roger Hewitt: White talk black talk: Inter-racial friendship and communication amongst adolescents, Cambridge: Cambridge University Press, 1986.

This is an important, very topical, well-researched and well-written contribution to the ethnography of adolescent cultures in our cities, and of creole speech among them. Hewitt has done intensive participant observation with two communities, labelled Area A and Area B, in South-East London, each containing a sizeable proportion of young people of Afro-Caribbean descent alongside the indigenous white teenagers. The two communities A and B are distinguished by their demography, social and economic structure and history. We are shown - with extensive verbatim transcriptions - how the teenagers talk about themselves and about the groups they perceive to exist around them. In the process we are led to an understanding of their cultures, the tensions and cohesive forces at work, the nature of their organisation, the roles played by various cultural stereotypes, and the role played by creole speech in the behaviour of white adolescents both among themselves and in inter-racial contexts. By contrasting the two communities along several parameters of social history and organisation, significant features are illustrated and contrasted with accepted stereotypes and with the fantasies of members of the groups themselves.

Within Area A two groups are compared: one of them based on the specific style of skinheads, the other based not on style but on friendship. The racial feelings expressed are not just those towards black people, but towards Asian immigrants as well. The expression of social stereotypes is seen as helping to diffuse them more widely through the networks within which each adolescent group operated, and then beyond; and the activities of each group are related to the networks which support them and to which they give support. These networks provide a focussing mechanism for adolescent language:

> 'The significance of the network in which this group was situated lay especially in that network's capacity, under certain conditions, to relay and boost these 'racial' attitudes and practices within itself, and to achieve a consensus of meanings and terms which spread beyond the network to penetrate, although unevenly, the wider and less formally coherent structures of adolescent association.' (p.29)

There is an important longitudinal element in Hewitt's research; it was spread over a period of two years or more, during which the same informants in each area were interviewed at intervals of not less than six months. The four interviews with each adolescent provided evidence of changes of attitudes and of friendship patterns. The

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communicative aspect in which Hewitt was especially interested, the use of creole by whites,`tended to occur in early teens. Informants were therefore, recruited from the thirteen to seventeen age-range.' (p.8)

Chapters 1 and 2 are concerned with inter-racial friendship in the two areas. Chapter 3 is headed `The language of black youth culture', Chapters 4 and 5 `Creole forms in white adolescent speech' and `White creole use in inter-racial contexts' respectively. The research period takes us up to 1984, and the changing influences of Jamaican, Rastafarian and American cultures are plotted. Hewitt's observations, and their implications, are important both for macrolinguistic and for microlinguistic studies - for example, the extent to which it is true that there is any discrete entity, London Jamaican, or any use of such an entity. Here again, Area A was to be distinguished from Area B:

> `From my own observation and interviews it appears that demographic factors are influential. In the areas of densest black settlement the lateral supports for creole use are inevitably greater.. In Area A there were many black youngsters who knew and used very little creole. Indeed, it was common for black adolescents from both lower-middle-class and working-class homes in the area not to use creole at all. In Area B the use of creole was substantially greater amongst adolescents, and was also supported more strongly by continuities with the adult population.. On the other hand, moderating parental influence also operated in both areas. Many black parents equate creole use with economic failure .. it was apparent that [in Area A], until attending secondary school and meeting more black children, many black youngsters had heard comparatively little creole .. School became, in fact, a major site of creole use and acquisition.' (pp.105-6)

Even in Area B many youngsters have a poor command of creole but, because of its identification with black culture, they are reluctant to confess to not understanding it. The symbolic importance of creole is very apparent.

Only a minority of whites use any creole. Nevertheless, creole influence was evident in the speech of some white children with other whites. It appears that some anglicised creole items have lost their ethnic marking in the speech of some London children. Other items are still marked for ethnicity. The exploration of creole forms in white adolescent speech is detailed and handled with considerable subtlety and discernment; as is white creole use in inter-racial contexts in the next chapter.

Chapter 6 is concerned with social semiotics and ideology, and Chapter 7 with 'Transmission and intervention: racism and anti-racism in communicative practices'. In Chapter 6, Hewitt rightly chides sociolinguists for the simplistic nature of many of our assumptions about objective social reality being directly related to subjective social assessments made by speakers and reflected in their speech. 'The notions of social class with which sociolinguists have worked.. have been amongst the least rigorous in the social sciences'. (pp 200-201) He explores the relationship between language use and concepts concerning race and ethnicity, as expressed by his informants, with great insight.

Chapter 7 returns us to the questions about the transmission of racism asked at the beginning of the book, examining in careful detail texts which yield information about the social stereotypes implicit in their language, and it contains what is for sociolinguists perhaps the most important paragraph in the book, which I feel I must quote in full: (p.223)

> 'The combination of a specific 'society' constituting such an 'other term', with the 'code' (formed here by racist notions and motifs) is in fact a feature of all codes, social, cultural and linguistic. Roman Jakobson's isolation of 'addresser' and 'addressee' within his well known account of the elements of communication (Jakobson 1960), involves, in this respect, a certain obfuscation of 'the social'. Just as his concept, 'message', necessarily involves the 'addresser'/'addressee' relation, so 'code' necessarily involves 'the social': both 'code' and 'message' are essentially dual-aspect entities; and if the inventory of elements necessary for communication should include 'addresser' and 'addressee' as well as 'message', it should also include 'society' as well as 'code', for 'addresser' and 'addressee' are to 'society' as 'message' is to 'code'. All meaning within language is only guaranteed by the accord of code and social 'knowledge'. 'Code' and 'society' are thus not independent terms. Where natural language is concerned, it is at least the case that the latter is entailed in the former.'

The book is about growing up in and growing into a society of strong racial prejudices; it is also about the fact that we live in networks of language, and become trapped in successive spiders' webs of stereotypes as we 'mature' - stereotypes about language as well as about race.

R B Le Page

Pacific Linguistics, ed S A Wurm, Series A No 65:
Papers in pidgin and creole linguistics No 3
Canberra: Australian National University,
Department of Linguistics
1983

This volume contains papers on Pacific pidgins and creoles ranging from Tsing-Tau, in central eastern China, to the Kimberleys and the Northern Territory of Australia; from Malacca to Papua-New Guinea and the Solomon Islands. The papers are all fairly strong on linguistic data; none more so than Lois Carrington's 80-page study which opens the volume, 'Eyewitness Reporting'. Forty pages of this consist of transcribed texts. Her paper and several of those which follow are directly, and interestingly, relevant to my own current obsessions with 'the standardization of languages and the vernacularization of literacy', and I should like to discuss their importance in this context.

Carrington's data consists of 4-minute written exercises in eyewitness reporting submitted by 160 of her Papua-New Guinea administrative trainees in October 1973, together with those submitted earlier by 80 of the same students in June 1973. The exercise consisted of observing a simple scenario connected with 'how to answer the telephone' and 'how to pass a message', and then writing down in Tok Pisin what had occurred. The trainees had a wide range of native Papua-New Guinea vernaculars; English and Tok Pisin were used both in their training and as lingua francas among them at the college. Most of their formal written textbooks were in English, but some less formal Tok Pisin written materials were available. A careful comparison of the June and October texts can tell us something

(a) about the development of their fluency in writing Tok Pisin

(b) about the interaction between Standard English orthography and usage and Tok Pisin orthography and usage

(c) about the development of norms of Tok Pisin usage among the students during their course, i.e. 'focussing' (see Le Page and Tabouret-Keller, 1985).

The analysis which follows the texts in Section 7, 'Putting the data to use', gives samples of ways in which one might learn. Paras 7.1 and 7.2, for example, analyse the use of the plural marker ol and/or -s plurals. 7.4 deals with spelling variations, listed by the number of occurrences of each; they cross the borderline between spelling variants and lexical variants, 'left hand' for example ranging from

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lefpela han to han kais. 7.5 gives lists of the idioms used to convey such stock meanings as 'the telephone rings', 'she put back the files', 'radio antenna or aerial'. The author does not pretend to be doing anything more than make suggestions, but the overall effect is far more important, since one is stimulated to think of ways in which the whole complex of factors involved in the development and interaction of vernacular norms in speech and writing might be studied.

Siegel's paper, Media Tok Pisin, opens with an almost classic statement for my purposes:

> 'One of the sociolinguistic changes that accompanies the process of creolisation is the extension of use of the language into new domains. In recent years Tok Pisin has become a widely used language in the mass media of Papua New Guinea... A certain amount of language engineering has gone into ... this wider usage and many changes have taken place in the language, both planned and unplanned, so that Media Tok Pisin can now be distinguished from other varieties. This paper looks at some of the factors... such as standardisation and choice of sociolect, the influence of the medium of writing, and linguistic expansion to meet requirements of both the medium and wider use.' (p.81)

His paper is exploratory and ends on as many questions as it answers, partly because of the rapid rate of change in Media Tok Pisin itself. Some of the examples of change are examples of the grammaticization of lexical items in the course of linguistic focussing, such as the development of ya (Sankoff and Brown, 1976, ia). Some illustrate the inherently distinct properties of a written language as opposed to a spoken language - multiple iteration for continuative aspect in the verb being replaced by simple reduplication, for example

Spoken TP	Mi ron yet i go i go i go i go I was still running and went on and on ...
Written script	Ol i mekim mekim They did it and did it ...

Some exemplify what has to be supplied in a written text where not only prosodic features but also body language are missing; some exemplify the influence of the knowledge of more Standard English among the educated who write the radio scripts and the advertisements. There have been conscious efforts to standardize Tok Pisin on the basis of the rural, rather than the Anglicised urban, dialects, and to keep the written variety more in step with the spoken. The paper is full of thought-provoking insights. The penultimate paragraph clearly illustrates the processes of focussing and diffusion:

> 'There seem to be two opposing forces acting on the language in its new role. The first is the conservative force resulting from the standardisation necessary for intelligibility in wider use. The second is the innovative force bringing about changes required by the wider use, such as adaptation to a new medium and linguistic expansion.' (p.90)

Mühlhäusler's first paper, 'Learning to speak about speaking in a pidgin language' insists to begin with that

> 'The grammar of a pidgin is shaped by two distinct groups of speakers:
>
> a) the socially dominant colonisers who speak a standard version of the pidgin's lexifier language.
>
> b) the subordinate indigenous population who speak many different vernaculars.
>
> As the pidgin language develops, the importance of the former decreases whilst that of the latter increases.
>
> The distinction between two groups of speakers, rather than the postulation of a unified speech community (as done for instance by Robert A. Hall 1943) is important for our purposes: How users of Tok Pisin speak the language, speak about the language and do things with the language depends very much on their group membership.
>
> This is particularly important for the expatriate linguist and language planner. The view that one is dealing with a single grammar has resulted in the failure to discover many aspects of grammar and is further responsible for the large number of unacceptable suggestions on language planning.' (p.94)

Mühlhäusler insists (quite rightly) that 'to understand the nature of a language like Tok Pisin it is essential not only to know its grammatical structures but also its external use, i.e. the communicative functions in which it is employed by its speakers'. What is special, he feels, about Tok Pisin is that it is very widely used as a lingua franca in severely restricted functions. (This is not confined to Tok Pisin however - LeP.) He will discuss how second-language Tok Pisin speakers have become able to carry out the function, not common in a pidgin, of talking about Tok Pisin in Tok Pisin.

Mühlhäusler traces the awareness of Tok Pisin, and of varieties within Tok Pisin, which has come since independence. The metalinguistic capacity of Tok Pisin speakers can be categorised in a tentative hierarchy:

'i) awareness of Tok Pisin as a language separate from English
ii) awareness of distinct varieties within Tok Pisin:

  a) indigenous vs. non-indigenous varieties
  b) socially determined varieties
  c) diachronic varieties (developmental stages)
  d) stylistic varieties' (p.98)

Categories (b) to (d), he says, seem to have emerged almost simultaneously between 1960 and 1970; during the same period Tok Pisin terms emerged describing language use (e.g. tok gris to flatter), and by the 1970's letters were appearing in the newspaper Wantok complaining about language which was either unintelligible or socially damaging. He comments that (p. 101) 'the latest, and as yet not fully developed domain within Tok Pisin metalinguistic language, are expressions referring to aspects of grammar ... It should also be noted that devices for talking about grammar depend, to a significant extent, on the institutionalisation and socialisation of rules of speaking'. This is an interim study, but contains fascinating observations on the relationship between various stereotypes about language and perceived linguistic behaviour.

Mühlhäusler's second paper is a discussion of 'The reality of Sapir's psychological reality of the phoneme'. It is a short critique of Sapir and of generativists who, like Chomsky, appear to accept Sapir's claims for his phonological orthography, illustrated with reference to variant spellings of words in Tok Pisin. He concludes, among other things, that 'speakers ... have at their disposal a number of different strategies to arrive at plausible spellings ... appear to be quite capable of hearing and representing subphonemic differences ... Instead of being discouraged by the many 'inconsistencies' in developing spelling systems, linguists should regard them as valuable evidence for both the language users' creativity and the linguistic variability of natural languages ...' (p.113). A number of other very interesting points are made, all of which have a bearing on 'idealising' linguistic theory and orthography.

The remaining six papers are rather more heterogeneous. Linda Simons makes a comparison between the pidgins of the Solomon Islands (Pijin) (rarely described) and of Papua-New Guinea (Tok Pisin). Before she can make her phonological, grammatical and lexical comparisons she draws our attention to orthographic similarities and differences, noting that 'it is difficult to talk about these [two phonologies] when we consider the wide dialectal variations in both languages. The orthographies for both languages are compromises, at best, in order to satisfy the need for standardisation in literature'.

Mühlhäusler's third paper is a short note on the Pidgin German of Kiatschon, a small area in the bay of Tsing-Tau in central eastern China which was a German protectorate from 1898 until 1914, by which time the whole population was 180,000 including about seven thousand Germans and German troops. The pidgin German which sprang up seems from the fragmentary remains to have been partly a re-lexification of a pre-existing pidgin English; it appears in any case to have been a fairly diffuse language.

Alan Baxter contributes a paper on Kristang (Malaccan Creole Portuguese) in relation to Creole universals as postulated, primarily, in Bickerton's *Roots of Language* (1981); with reference also to Malacca Bazaar Malay as a kind of control for possible sources of features in Kristang. There is a careful analysis of a good deal of data; the degree of support for Bickerton's hypotheses is somewhat muted.

Joyce Hudson's paper is on Transitivity and Aspect in the Kriol Verb, Kriol in this case being ‘an English-based language of the Northern Territory and Kimberley area of Western Australia’. We have here a useful, straightforward and carefully exemplified piece of grammatical description, with an introductory section on the morphophonemics.

Alan Rumsey's ‘On some syntactico-semantic consequences of homophony in North-West Australian Pidgin/Creole English’ refers back (as does Joyce Hudson's paper) to the Sandefurs' descriptions of the Ngukurr-Bamyili dialects of Northern Territory Creole; he wishes ‘to supplement their account with some observations based on my own field experience of the same creole, and an analysis of published texts, and to draw out some of their implications for post-structuralist linguistic theory’. He draws attention to the homophonies that would theoretically result from the fact that, for example, the voiced and voiceless contrasts of English were neutralised in the Pre-Kriol, fricatives and affricates became stops, and the vowel inventory was reduced to five. There are however compensatory features at levels other than phonology, which greatly reduce the homophony in practice. For example, transitivity is marked in Kriol by a verbal suffix; past tense by auxiliary bin; adjectives by a suffix; the range of meanings of lexical items differs in Kriol from English.

Rumsey notes (p.182) ‘one of the most frequent and interesting kinds of semantic shift that English-derived lexicon has undergone during pidginisation/creolisation has been the conflation of etymologically distinct homophones’, and comments that this phenomenon has not been extensively treated within pidgin/creole studies. It is however invoked explicitly in the *Dictionary of Jamaican English* and in several of F G Cassidy's prior papers. A good Jamaican example is the conflation of Twi doti, ‘yard around the house', with Creole doti, reflex of English dirty, leading to Jamaican Creole auta doti, 'earth outside'. What Rumsey finds even more frequent are conflations of two words both of English origin, so that Kriol jigi (bala) = English *sticky* and *cheeky*. 'This word is very frequently used in the sense of 'cheeky' ... it describes, for instance, poisonous snakes, strong alcoholic beverages, and spicy foods. By at least some speakers in the Kimberley region, it has been used to describe heated spinifex resin, which is used as a glue in traditional technology.'

Rumsey claims that 'such conflations are of great theoretical interest, for at least three reasons.' (p.183) 'First, they show that languages tend toward a state which we can describe by the slogan "one signifier - one signified". This is something which Saussure noticed long ago ...'. To me, Saussure's statement has always seemed to betray a basic misunderstanding as to the nature of semiotic systems, and I certainly cannot see that Rumsey's evidence supports it; rather the opposite. How can we know how many "signifieds" we have, as long as metaphorical extension and analogy can be pushed still further; how can we say, until we take the arbitrary decisions that a lexicographer must take, 'now we have a new meaning, and therefore a new word'? Rumsey ends on a questioning note, of how one can relate systems with such inherent variability as is found among the Fitzroy Crossing Creole speakers he has been describing, to any kind of Saussurean *langue*; wisely, he decides not to try to answer the question here.

Ann Chowning's account is of 'Interaction between Pidgin and three West New Britain languages', and she begins by drawing attention to regional variation within rural varieties of Tok Pisin. She goes on to examine the further variations she has encountered in West New Britain, and to relate these both to the three indigenous Austronesian languages she learned while there: Lakalai, Sengseng and Kove; and to the role of Pidgin within each of these cultures. Her paper provides many interesting examples of the multifaceted processes of linguistic interaction between communities, particularly at the lexical level.

A volume of studies which seems at first sight rather heterogeneous proves to offer many important insights into linguistic evolution; it is the kind of volume every linguist should read, and ask himself, whether his particular theory of language, of language acquisition and language transmission, can accommodate the data so profusely here on offer. If it can't, he should probably throw his theory out of the window.

R B Le Page

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